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ART. I.—*Second Letter to Prof. Faraday;* from R. HARE, M. D., Professor of Chemistry in the University of Pennsylvania.

Philadelphia, February, 1841.

TO PROF. FARADAY.

My dear Sir—IN the month of July last I had the pleasure to read, in the American Journal of Science, your letter in reply to one which I had addressed to you through the same channel. I should sooner have noticed this letter, but that meanwhile I have had to republish two of my text-books, and, besides, could not command, until lately, a complete copy of all those numbers of your researches to which you have referred.

The tenor of the language with which your letter commences realizes the hope, which I cherished, that my strictures would call forth an amicable reply. Under these circumstances it would grieve me that you should consider any part of my language as charging you with inconsistency or self-contradiction, as if it could be my object to put you in the wrong, farther than might be necessary to establish my conception of the truth. Certainly it has been my wish never to go beyond the sentiment, "*Amicus Plato, sed magis amica veritas.*" I attach high importance to the facts established by your "*Researches,*" which can only be appreciated sufficiently by those who have experienced the labor, corporeal and mental, which experimental investigations require. I am moreover grateful for the disposition to do me justice, manifested in those researches; yet it may not always be possible for me to display the deference, which I nevertheless entertain. I am

aware that when in a discussion, which due attention to brevity must render unceremonious, diversities of opinion are exhibited, much magnanimity is requisite in the party whose opinions are assailed; but I trust that both of us have truth in view above all other objects, and that so much of your new doctrine as tends to promote that end, will not be invalidated by a criticism which though free, is intended to be perfectly fair and friendly.

In paragraph (ii,) your language is as follows, "*my theory of induction makes no assertion as to the nature of electricity, nor at all questions any of the theories respecting that subject.*" Owing to this avowed omission to state your opinions of the nature of electricity as preliminary to the statement of your "*theory*," and because I was unable to reconcile that theory with those previously accredited, I received the impression that you claimed no aid from any imponderable principle. It appeared to me that there was no room for the agency of any such principle, if induction were an *action* of contiguous ponderable particles, *consisting* of a species of polarity. It seemed to follow, that what we call electricity, could be nothing more than a polarity, in the ponderable particles, directly caused by those mechanical or chemical frictions, movements, or reactions by which ponderable bodies are electrified. You have correctly inferred that I had not seen the fourteenth series of your researches, containing certain paragraphs. From them it appears that the polarity, on which so much stress has been laid, is analogous to that which has long been known to arise in a mass, about which the electric equilibrium has been subverted, by the inductive influence of the electricity accumulated upon another mass. This is clearly explained in paragraph iv of your letter, by the illustration, agreeably to which three bodies, A, B, C, are situated in a line, in the order in which they are named, in proximity, but not in contact. "A is electrified positively and then C is uninsulated." It is evident that you are correct in representing that under these circumstances the extremities of B will be oppositely excited, so as to have a reaction with any similarly excited body, analogous to that which takes place between magnets; since the similarly excited extremities of two such bodies, would repel each other; while those dissimilarly excited, would be reciprocally attractive. Hence no doubt the word polarity is conceived by you to convey an idea of the state of the body B. If I may be allowed to propose an

epithet to convey the idea which I have of the state of a mass thus electrified, I would designate it as an electropolar state, or as a state of electropolarity.

It does not appear to me that in the suggestion of the electropolarity which we both agree to be induced upon the body B, (iv,) so long as it concerns a mass, there is any novelty. The only part of your doctrine which is new, is that which suggests an analogous state to be caused in the particles of the bodies through which the inductive power is propagated. Admitting each of the particles of a dielectric, through which the process of ordinary induction takes place, to be put into the state of the body B, it does not appear to me to justify your definition of electrical induction. I conceive that consistently with your own exemplification of that process, you should have alleged ordinary induction to be *productive* of an *affection* of particles *causing* in them a species of polarity. In the case of the bodies, A, B, C, (iv,) B is evidently passive. How then can we consider as active, particles represented to be in an analogous state? If in B there is no action, how can there be any action in particles performing a perfectly similar part? Moreover, how can the inductive power of an electrical accumulation upon A, *consist* of the polarity which it induces in B?

Having supposed (viii,) an electrified ball, A, an inch in diameter, to be situated within a thin metallic sphere, C, of a foot in diameter, you suggest that were one thousand concentric metallic spheres interposed between A, and the inner surface of C, the electropolar state of each particle in those spheres would be analogous to that of B already mentioned. Of course if there be an action of those particles, there must be an action of B; but this appears to me not only irreconcilable with any previously existing theory, but also with your own exposition of the process by which B is polarized.

Supposing concentric metallic hemispheres were interposed only upon one side of A, you aver that agreeably to your experience, more of the inductive influence would be extended towards that side of the containing shell than before, (xiv.) Admitting this, I cannot concede that the greater influence of the induction, resulting from the presence of the metallic particles, is the consequence of any *action* of theirs; whether in *contiguity* or in *prox-*

imity. Agreeably to my view, the action is confined to the electrical accumulation in the sphere A. Between the electricity accumulated in this sphere, and that existing in, or about, the intervening ponderable particles, there may be a reaction; but evidently these particles are as inactive as are the steps of a ladder in the scaling of a wall.

Suppose a powerful magnet to be so curved as to have the terminating polar surfaces parallel, and leaving between them an interval of some inches. Place between these surfaces, a number of short pieces of soft iron wire. These would of course be magnetized, and would arrange themselves in rows, the north and south poles becoming contiguous. Would this be a sufficient reason for saying that the inductive influence of the magnetic poles was an *action* of the contiguous wires? Would not the phenomena be the consequence of an affection of the contiguous pieces of wire, not of their action?

As respects the word charge, I am not aware that I have been in the habit of attaching any erroneous meaning to it, as your efforts to define it in paragraph iii would imply. I have been accustomed to restrict the use of it to the case which you distinguish as an inductive charge, illustrated by that of the Leyden jar. To designate the states of the conductors of a machine, I have almost always employed the words *excited* or *excitement.* In my text-book, these words are used to designate the state of glass or resin electrified by friction, while that of coated surfaces, whether panes or jars, inductively electrified, has been designated by the words *charge* or *charged.*

I understood the word contiguous to imply contact, or contiguity, where, as it seems that it was intended by you to convey the idea of proximity. In the last mentioned sense it is not inconsistent with the idea of an action at the distance of half an inch: but by admitting the word contiguous to be ill chosen, you have, with great candor, furnished me with an apology for having mistaken your meaning.

Any inductive action which does not exist at sensible distances, (xx,) you attribute to *ordinary* induction, considering the case of induction through a vacuum as an *extraordinary* case of induction. To me it appears that the induction must be the same in both cases, and that the *circumstances* under which it acts, are

those which may be considered in the one case as *ordinary*, in the other, *extraordinary*. Thus, take the case cited in your reply, (viii, ix, x.) Does the interposition of the spheres alter the character of the inductive power in the sphere A?

Either the force exercised by the charge in A, is like that of gravitation, altogether independent of the influence of intervening bodies; or, like that of light, it is dependent on the agency of an intervening matter. Agreeably to one doctrine, the matter, by means of which luminous bodies act, operates by its transmission from the luminous surface to that illumined. Agreeably to another doctrine, the illuminating matter operates by its undulations. If the inductive power of electrified bodies be not analogous to gravitation, it must be analogous to the power by which light is produced so far as to be dependent on intervening matter. But were it to resemble gravitation, like that force it would be uninfluenced by such matter. If your experiments prove that electrical induction is liable to be modified by intervening matter, it is demonstrated that in its mode of operation it is analogous to light, not to gravitation. It is then proved, that, agreeably to your doctrine, electrical induction requires the intervention of matter, but you admit that it acts across a vacuum, and of course, acts without the presence of *ponderable* matter. Yet it requires intervening matter of some kind, and, since that matter is not ponderable, it must of necessity be imponderable. When light is communicated from a luminous body in the centre of an exhausted sphere, agreeably to the undulatory hypothesis, its efficacy is dependent on the waves excited in an intervening imponderable medium. Agreeably to your electropolar hypothesis, the inductive efficacy of an electrified body in an exhausted sphere would be due to a derangement of electric equilibrium, by which an electric state opposite to that at the centre would be produced at the surface of the containing sphere, (xxvi, xxvii.) This case you consider as one of extraordinary induction, but when air is admitted into the hollow sphere, or when concentric spheres are interposed, you hold it to be a case of ordinary induction. Let us then, in the case of the luminous body, imagine that concentric spheres of glass are interposed, of which the surfaces are roughened by grinding. In consequence of the roughness thus produced, the rays instead of proceeding in radii from the central

ball would be so refracted as to cross each other. Of the two instances of illumination, thus imagined, would the one be described as *ordinary*, the other as *extraordinary radiation?* But if these epithets are not to be applied to radiation, wherefore under analogous circumstances are they applicable to induction? Wherefore is induction when acting through a plenum to be called ordinary, and yet when acting through a vacuum to be called extraordinary? In the well known case of the refracting power of Iceland spar, light undergoes an *ordinary* and *extraordinary refraction;* not an *ordinary* and *extraordinary radiation.* The candle, of which, when viewed through the spar, two images are seen, does not *radiate ordinarily* and *extraordinarily.*

If there be occasionally, as you allege, (xxi,) large intervals between the particles of radiant heat, how can the distances between them resemble those existing between particles acting at distances which are not sensible. The repulsive reaction between the particles of radiant caloric, as described by you, (xxi,) resembles that which I have supposed to exist between those of electricity; but I cannot conceive of any description less suitable for either, than that of particles which do not act at sensible distances.

Aware that the materiality of heat, and the Newtonian theory, which ascribes radiation to the projection of heat or light producing particles, have been questioned, I should not have appealed to a doctrine which assumes both the materiality of heat, and the truth of the Newtonian theory, had not you led the way; but, agreeably to the doctrine and theory alluded to, I cannot accord with you in perceiving any similitude between the processes of conduction and radiation.

Consistently with the hypothesis that electricity is material, you have shewn that an enormous quantity of it must exist in metals. To me it seems equally evident that, agreeably to the idea that heat is material, there must exist in metals a proportionably great quantity of caloric. The intense heat produced when wires are deflagrated by an electrical discharge, cannot otherwise be consistently accounted for. Agreeably to the same idea, every metallic particle in any metallic mass, must be surrounded by an atmosphere of caloric; since the *changes* of dimension consequent to variations of temperature, can only be explained by corresponding variations in the quantity of caloric imbibed, and in

the consequent density of the calorific atmospheres existing in the mass which undergoes these changes.*

Such being the constitution of expansible bodies, agreeably to the hypothesis in question, it seems to me that the process, by which caloric is propagated through them by *conduction*, must be extremely different from that by which it is transmitted from one part of space to another by *radiation*. In the one case the calorific particle flies, like a cannon ball, with an inconceivably greater velocity, which is not sensibly retarded by the reflecting or refracting influence of intervening transparent media: in the other case it must be slowly imparted from one calorific atmosphere to another, until the repulsion sustained on all sides is in equilibrio. It is in this way that I have always explained the fact that metals are bad radiators, while good reflectors.†

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*I subjoin the language which I have held respecting the constitution of expansible solids, during the last twenty years.

"The expansion of matter, whether solid, liquid, or aëriform, by an increase of temperature, may be thus explained.

"In proportion as the temperature within any space is raised, there will be more caloric in the vicinity of the particles of any mass contained in the space. The more caloric may abound in the vicinity of the particles, the more of it will combine with them; and in proportion to the quantity of caloric thus combined, will they be actuated by that reciprocally repellent power, which, in proportion to its intensity, regulates their distance from each other.

"There may be some analogy between the mode in which each ponderable atom is surrounded by the caloric which it attracts, and that in which the earth is surrounded by the atmosphere; and as in the latter case, so probably in the former, the density is inversely as the square of the distance.

"At a height at which the atmospheric pressure does not exceed a grain to the square inch, suppose it to be doubled and supported at that increased pressure by a supply of air from some remote region; is it not evident that a condensation would ensue in all the inferior strata of the atmosphere, until the pressure would be doubled throughout, so as to become at the terrestrial surface, 30 lbs. instead of the present pressure of 15 lbs.? Yet the pressure at the point from which the change would be propagated, would not exceed two grains per square inch. In like manner, it may be presumed that the atmospheres of caloric are increased in quantity and density about their respective atoms, by a slight increase in the calorific tension of the external medium."

†I will here quote the rationale which has been given in my lectures for the last twenty years. "Metals appear to consist of particles so united with each other, or with caloric, as to leave no pores through which radiant caloric can be projected. Hence the only portion of any metallic mass which can yield up its rays by radiation, is the external stratum.

"On the other hand, from its porosity, and probably also from its not retaining caloric within its pores tenaciously as an ingredient in its composition, charcoal opposes but little obstruction to the passage of that subtile principle, when in the radiant form; and hence its particles may all be simultaneously engaged in radiating

In paragraph (xxv,) you allege that conduction of heat differs from electrical induction, because it passes by a very slow process; while induction is in its distant influence simultaneous with its force at the place of action. How then can the passage of heat by conduction, be "*a process precisely like that of radiation*," (xxi,) which resembles induction in the velocity with which its influence reaches objects, however remote?

Although, (xxi,) you appeal to the "modern views respecting radiation and conduction of heat," in order to illustrate your conception of the contiguity of the particles of bodies subjected to induction, yet in (xxv,) you object to the reference which I had made to these views, in order to shew that the intensity of electropolarization could not be inversely as the number of particles interposed between the "inductric" surfaces. Let us then resort to that above suggested, of the influence of the poles of a magnet upon intervening pieces of iron wire. In 1679, 14th series, you suggest this as an analogous case to that of the process of *ordinary* electrical induction, which we have under consideration. Should there be in the one case a thousand pieces of wire interposed, in the second an hundred, will it be pretended that the intensity of their reciprocal inductive reaction would be inversely as the number; so that the effect of the last mentioned number of wires would be *equivalent* to that of the first? Were intervals to be created between the wires by removing, from among the number first mentioned, alternate wires, it would seem to me that the diminution of effect would be commensurate not only with the reduction of the number of the wires, but likewise with the consequent enlargement of the intervals.

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any excess of this principle with which a feeble affinity may have caused them to be transiently united, or in receiving the rays emitted by any heated body, to the emanations from which they may have been exposed.

"We may account, in like manner, for the great radiating power of earthen ware and wood.

"For the same reason that calorific rays cannot be projected from the interior of a metal, they cannot enter it when projected against it from without. On the contrary, they are repelled with such force as to be reflected without any perceptible diminution of velocity. Hence the pre-eminence of metallic reflectors.

"It would seem as if the calorific particles which are condensed between those of the metal, repel any other particles of their own nature which may radiate towards the metallic superficies, before actual contact ensues; otherwise, on account of mechanical imperfections, easily discernible with the aid of a microscope, mirrors would not be as efficacious as they are found to be in concentrating radiant heat. Their influence, in this respect, seems to result from the excellence of their general contour, and is not proportionably impaired by blemishes."

If as you suggest, the interposition of ponderable particles have any tendency to promote inductive influence, (xiv,) there must be some number of such particles by which this effect will be best attained. That number being interposed, I cannot imagine how the intensity of any electro-polarity, thus created in the intervening particles, can, by a diminution of their number, acquire a proportional increase; evidently in no case can the excitement in the particles exceed that of the "inductric" surfaces whence the derangement of electrical equilibrium arises.

The repulsive power of electricity being admitted to be inversely as the squares of the distances, you correctly infer that the aggregate influence of an electrified ball, B, situated at the centre of a hollow sphere, C, will be a constant quantity, whatever may be the diameter of C. This is perfectly analogous to the illuminating influence of a luminous body situated at the centre of a hollow sphere, which would of course receive the whole of the light emitted whatever might be its diameter, provided that there were nothing interposed to intercept any portion of the rays. But in order to answer the objection which I have advanced, that the diminution of the density of a "*dielectric*" cannot be compensated by any consequent increase of inductive intensity, it must be shown in the case of several similar hollow spheres, in which various numbers of electrified equidistant balls should exist, that the influence of such balls upon each other, and upon the surfaces of the spheres, would not be directly as the number of the balls and inversely as the size of the containing spaces. Were gas lights substituted for the balls, it must be evident that the intensity of the light, in any one of the spheres, would be as the number of lights which it might contain. Now one of your illustrations (viii,) above noticed makes light and electrical induction, obey the same law as respects the influence of distance upon the respective intensities.

From these considerations, and others above stated, I infer, that if electrical induction were an action of particles in proximity operating reciprocally with forces varying in intensity with the squares of the distances, their aggregate influence upon any surfaces, between which they might be situated, would be proportionable to their number; and since experience demonstrates that the inductive power is not diminished by the reduction of the number of the intervening particles, I conclude that it is in-

dependent of any energy of theirs, and proceeds altogether from that electrical accumulation with which the inductive change is admitted to originate.

In paragraph (xxxi,) you say "that at one time there was a distinction between heat and cold. At present that theory is done away with, and the phenomena of heat and cold are referred to the same class, and to different degrees of the same power."

In reply to this I beg leave to point out, that although, in ordinary acceptation, cold refers to relatively low temperature; yet we all understand that there might be that perfect negation of heat, or abstraction of caloric, which may be defined absolute cold. I presume that, having thus defined absolute cold, you would not represent it as identical with caloric. For my own part this would seem as unreasonable as to confound matter with nihility.

Assuming that there is only one electric fluid, there appears to me to be an analogy between caloric and electricity, so far that negative electricity conveys, in the one case, an idea analogous to that which cold conveys in the other. But if the doctrine of Du Fay be admitted, there are two kinds of electric matter, which are no more to be confounded than an acid and an alkali. Let us, upon these premises, subject to further examination your argument (1330,) that insulation and conduction should be identified, "*since the moment we leave in the smallest degree perfection at either extremity, we involve the element of perfection at the opposite end.*" Let us suppose two remote portions of space, one, replete with pure vitreous electricity, the other with pure resinous: let there be a series of like spaces containing the resinous and vitreous electricities in as many different varieties of admixture, so that in passing from one of the first mentioned spaces, through the series to the other, as soon as we should cease to be exposed to the vitreous fluid, in perfect purity, we should begin to be exposed minutely to the resinous, or that, in passing from the purely resinous atmosphere, we should begin to be exposed to a minute portion of the vitreous fluid; would this be a sufficient reason for confounding the two fluids, and treating the phenomena to which they give rise as the effect of one only?

But the discussion, into which your illustrations have led me, refers to things, whereas conduction and insulation, as I understand them, are opposite and incompatible properties, so that,

in as much as either prevails, the other must be counteracted. Conduction conveys to my mind the idea of *permeability* to the electric fluid, insulation that of *impermeability;* and I am unable to understand how these irreconcilable properties can be produced by a difference of degree in any one property of electrics and conductors.

If, as your infer, glass have, comparatively with metals, an almost infinitely minute degree of the conducting power, is it this power which enables it to prevent conduction, or in other words to insulate? Let it be granted that you have correctly supposed conduction to comprise both induction and discharge, the one following the other in perfect conductors within an inexpressibly brief interval. Insulation does not prevent induction; but, so far as it goes, it prevents discharge. In practice this part of the process of conduction does not take place through glass during any time ordinarily allotted to our experiments, however correct you may have been in supposing it to have ensued before the expiration of a year or more in the case of the tubes which you had sealed after charging them. But conceding it to have been thus proved that glass has, comparatively with metals, an infinitely small degree of the conducting power; is it this minute degree of conducting power, which enables it to prevent conduction, or in other words to insulate?

Induction arises from one or more properties of electricity, insulation from a property of ponderable matter; and although there be no matter capable of preventing induction, as well as discharge, were there such a matter, would that annihilate insulation? On the contrary would it not exhibit the property in the highest perfection?

As respects the residual charge of a battery, is it not evident that any electrical charge which affects the surface of the glass, must produce a corresponding effect upon the stratum of air in contact with the coating of the glass? If we place one coating between two panes, will it not enable us to a certain extent to charge or discharge both? Substituting the air for one of them, will it not, in some measure, be liable to an affection similar to that of the vitreous surface for which it is substituted? In the well known process of the condensing electromoter, the plate of air interposed between the disks is, I believe, universally admitted to perform the part of an electric, and to be equivalent in its properties to the glass in a coated pane.

When I adverted to a gradual relinquishment of electricity by the air to the glass, I did not mean to suggest that it was attended by any more delay than the case actually demonstrates. It might be slow or gradual, compared with the velocity of an electric discharge, and yet be extremely quick, comparatively with any velocity ever produced in ponderable matter. That the return should be slow when no coating was employed, and yet quick when it was employed, as stated by you, (xxxviii.) is precisely what I should have expected; because the coating only operates to remove all obstruction to the electric equilibrium. The quantity or intensity of the excitement is dependent altogether upon the electrified surfaces of the air and the glass. You have cited (1632,) the property of a charged Leyden jar, as usually accoutered, of electrifying a carrier ball. This I think sanctions the existence of a power to electrify by "convection," the surrounding air to a greater or less depth; since it must be evident that every aërial particle must be competent to perform the part of the carrier ball.

Agreeably to the Franklinian doctrine, the electricity directly accumulated upon one side of a pane repels that upon the other side. You admit that this would take place were a vacuum to intervene; but when ponderable matter is interposed, you conceive each particle to act as does the body B when situated as described between A and C, (iv.) But agreeably to the view which I have taken, and what I understand to be your own exposition of the case, B is altogether passive, so that it cannot help, if it does not impede the repulsive influence. Moreover it must be quite evident, that were B removed, and A approximated to C, without attaining the striking distance, the effect upon C and the consequent energy of any discharge upon it from A, would be greater instead of less. If in the charge of a coated pane the intermediate ponderable vitreous particles have any tendency to enhance the charge, how happens it that, the power of the machine employed being the same, the intensity of the charge which can be given to an electric is greater in proportion to its tenuity?

In reference to the direction of any discharge, it appears to me that as, in *charging*, the fluid must always pass from the cathode to the anode, so in reversing the process it must pursue, as I have alleged, the opposite course of going from the anode back to the cathode. Evidently the circumvolutions of the circuit are as un-

important as respects a correct idea of the direction, as their length has been shown by Wheatstone, to be incompetent to produce any perceptible delay.

The dissipation of conductors being one of the most prominent among electrical phenomena, it appears to me to be an objection to your theory, if, while it fails to suggest any process by which this phenomenon is produced, it assumes premises which seem to be incompatible with the generation of any explosive power. If discharge only involves the restoration of polarized ponderable particles to their natural state, the potency of the discharge must be proportionable to the intensity of the antecedent polarity; yet it is through conductors, liable, as you allege, to polarization of comparatively low intensity, (xxxi,) that discharge takes place with the highest degree of explosive violence.

Having inquired how your allegation could be true, that discharge brings bodies to their natural state and yet causes conductors to be dissipated, you reply (xxxiv,) that different effects may result from the same cause acting with different degrees of intensity; as when by one degree of heat ice is converted into water, by another into steam. But it may be urged, that although in the case thus cited, different effects are produced, yet that the one is not inconsistent with the other, as were those ascribed to electrical discharges. It is quite consistent, that the protoxide of hydrogen which *per se* constitutes the solid called ice, should by one degree of calorific repulsion have the cohesion of its particles so counteracted as to be productive of fusion; and yet that a higher degree of the same power should impart to them the repulsive quality requisite to the aëriform state. In order to found upon the influence of variations of temperature, a good objection to my argument, it should be shown, that while a certain reduction of temperature enables aqueous particles to indulge their innate propensity to consolidation, a still further reduction will cause them, in direct opposition to that propensity, to repel each other so as to form steam.

In your concluding paragraph you allege, "that when ponderable particles intervene, during the process of dynamic induction, the currents resulting from this source do require these particles." I presume this allegation is to be explained by the conjecture made by you (1729) that since certain bodies when interposed did not interfere with dynamic induction, therefore

they might be inferred to cooperate in the transmission of that species of inductive influence. But if the induction takes place without the ponderable matter, is it right to assume that this matter *aids* because it does not prevent the effect? Might it not be as reasonably inferred in the case of light, that although its transmission does not require the interposition of a pane of glass, yet that when such a pane is interposed, since the light is not intercepted, there is reason to suppose an active cooperation of the vitreous particles in aid of the radiation? It may be expedient here to advert to the fact that Prof. Henry has found a metallic plate to interfere with the dynamic induction of one flat helix upon another. I have myself been witness of this result.

Does not magnetic or electrodynamic induction take place as well in vacuo as in pleno? Has the presence of any gas been found to promote or retard that species of reaction? It appears, that agreeably to your experiments, ponderable bodies, when made to intervene, did not enhance the influence in question; while in some of those performed by Henry it was intercepted by them. Does it not follow that ponderable particles may impede, but cannot assist in this process.

I am happy to find, that among the opinions which I expressed in my letter to you, although there are several in which you do not concur, there are some which you esteem of importance, though you do not consider yourself justified in extending to them your sanction; being constrained, in the present state of human knowledge, to hold your judgment in suspense. For the present, I shall here take leave of this subject, having already so extended my letter as to occupy too much of your valuable time. I am aware that as yet I have not sufficiently studied many of the results of your sagacity, ingenuity, and skill in experimental investigations. When I shall have time to make them the subject of the careful consideration which they merit, I may venture to subject your patience to the additional trial resulting from some further commentaries. I remain, with the highest esteem, respectfully yours,

ROBERT HARE.

ART. II.—*On the Magnetical Dip in the United States;* by JOHN LOCKE, M. D., Prof. Chem. and Pharm., in the Medical College of Ohio.

TO THE EDITORS.

IN reply to the article by Prof. Loomis, in Vol. XXXIX of your Journal, permit me to observe by way of narration, that the correspondence between him and myself, originated in the following manner. Prof. Loomis being about to republish a chart of the magnetism of the United States, requested as a favor, that I would furnish him with some of the results of my observations. I cheerfully complied, by sending him the principal ones. When his paper appeared, I saw my results marked "in error" to a considerable amount. Upon examination, I found that the expression "in error," meant merely that the observations did not agree with an ASSUMED HYPOTHESIS, by the amount noted. I objected to this mode of expressing differences, chiefly because it would mislead popular readers, and with them would do me discredit as an observer. My paper on that subject in your Journal, Vol. XXXVIII, will be found at every point, complimentary to Prof. Loomis. The reader will please to observe that I have not made a single remark on Prof. L.'s own observations. I should have felt great delicacy, especially in undertaking to criticise or point out any errors or faults which I might suppose attended them. This is a task which I conceive belongs to the observer himself, after he shall have made all of the comparisons necessary for the labor, and shall have ascertained all of the circumstances possible, which may explain the real or apparent anomalies. A second person may misapprehend many things which the observer himself could have rectified, or explained instantly, and may thus produce injury of reputation, and of feeling, by hastily publishing to the world, a supposed discovery of errors, where none ever existed. I have merely sought that my own observations should be presented to the popular reader in such a manner that he should not misapprehend them to my discredit. Prof. Loomis will certainly grant that the notes of an unfinished labor of mine were communicated to him, with a tacit understanding to that effect. I did not in my last paper, presume that he had intended to use them in any other manner, than that which should be to my honor.

In the paper before me, Prof. Loomis makes a distinct announcement of the "hypothesis" in the following words: Are these differences to be regarded as errors of observation, or as errors of the *hypothesis of parallel, straight, and equidistant isoclinal lines?** That is *the* question, which would have given a clear commencement to the subject. Had the reader been advised that it was upon grounds purely *hypothetical*, that my observations were pronounced in error, it was all I desired. Had Prof. Loomis stopped at this point, the whole thing would have been satisfactory to me; but in the latter part of his last paper, he has made a special *effort* to discredit my observations. I have great dislike to even the *appearance* of controversy, especially in such a journal as yours, and were it not that some instruction may arise to your readers, I would suppress the following remarks. I have been drawn into the subject unintentionally, by an attempt to serve a friend.

In the first place, I question the above quoted "hypothesis" itself. It is certainly anti-Baconian, to assume an hypothesis, and then require observed facts to be stretched out, or cut off, by + or −, until they agree with the assumption. We propose first to consider the consistency of the hypothesis itself, as used by Prof. Loomis, in his last paper, and next to reply to his special pleadings, to sustain that hypothesis, in which he endeavors to discredit the facts observed by me. In the first group of observations, consisting of mine and his together, he "adopts as" a central position, "lat. 41° 22′, long. 84° 54′," and by the proper formulæ, determines "the direction of the isoclinal lines* to be N. 80.1′ W." "*Computing* from these data, the dip at several stations," he obtains the so called "errors of observation." Here we have first the general "hypothesis," and secondly an assumed "central position."

In the second group of observations, consisting of those made by myself in the region of the Mississippi, the central position adopted, is "lat. 42° 00′ N. long. 90° 10′ W." By the formulæ used above, he determines the direction of the isoclinal lines in this group, to be "N. 65° 01′ W." Again, from these data, he *computes* the dip, and determines the so called "errors of observation." Agreeably to these *calculations*, the lines of equal dip,

* Lines of equal dip.

in the two groups, have bearings differing 19° 53′ from each other. What will be the bearings of those lines in the intermediate region? Ought the two lines to be produced, the one from Iowa, and the other from Ohio, until they meet? The *two* would not certainly form *one* "*straight line*," which is called for by the hypothesis. Or, ought there to be an intermediate "central position adopted," and a new bearing of the isoclinal line determined? This would likely give us a polygon. Now "adopt," or assume an infinite number of "central positions," and calculate as many bearings of the isoclinal or equal dip lines, and the result will be *curves*, precisely what I contend for. But this is contrary to the "hypothesis," which calls for "straight lines." Here might be a very great modification of the "computed errors of observation." My own opinion, founded on observations of considerable extent, is that the lines do proceed in curves which, in the present state of our knowledge, can be traced only by observation. The hypothesis that the magnetical forces proceed in lines, less tortuous in their course than observations would seem to indicate, is one by no means of recent date; for it is found in the philosophical writings of the Baron Swedenborg, together with a very specific account of the sources of error, in making observations. The appearance of mathematical calculation, especially under the symbols of modern analysis, is very imposing to many a popular reader, who may not reflect that all mathematical results are dependent on the *data*, the *assumptions*, the *hypotheses;* if these be false or erroneous, the superstructure is but a wreath of mist. Grant the data, and the calculations in these times, are but a schoolboy's task.*

On page 87, Prof. Loomis observes in reference to my observations, as treated in his previous paper, "at Prairie du Chien, the discordance is more considerable. The difference [from the hypothesis] I now find is $-7'.3$; in my former paper $-20'$. The discordance is owing in part to the *curvature I ascribed* to the isoclinal lines, by which most of the observations seemed best represented, though the apparent error of this observation was increased." All this is perfectly candid. He admits a curvature in the "straight lines," and that the lines are those of his own creation, subject to be *changed* at pleasure; not the isoclinal

* It is evident from the manner in which Prof. L. uses the hypothetical straight lines, that they were not really the lines of equal dip, but that they were tangents to those lines.

lines marked by the Architect of the Universe. My observations were intended to be tried by no other standard than the last.

I come next to Prof. Loomis's remarks against the truth of my observations.

On page 88, he doubts whether I have attended to reading both ends of the dipping needle, so as to avoid the error "arising from the eccentricity of the axis of the needle in relation to the vertical circle, on which the readings are made." He says, "this error in my instrument, commonly amounts to one or two minutes, and sometimes to even five or more. It is corrected by reading at both extremities of the needle." From my saying that I determined the dip by 8 distinct readings, he thinks I "implied that I did not attend to the above precaution." This is all *naivete* enough. I regret that Prof. Loomis should have formed so low an estimate of my skill, as to suppose I would use a graduated circle, where it was possible to obtain the mean of two opposite readings without doing so,—a thing so obvious that I deemed it unnecessary to say any thing about it. My needle No. 1, when properly adjusted, shows no eccentricity, but reads alike at both extremities. No. 2, has almost uniformly a difference of 5 minutes at the opposite ends. Both ends of both needles were always observed. If such be called a separate reading, then 16 should be substituted for 8 in the quotation above, and my readings should all be counted double the number which I have assigned to them.

The next paragraph is upon "the uncertainty of the readings themselves." I should not have known how to reply to that expression, but he subsequently becomes more specific. "The error arising from friction on the axis," he says, "is exhibited in Prof. Locke's observations in a *striking light*." I find by the subsequent remarks that he refers to an observation at Davenport, as published in Vol. xxxix, of this Journal, in which there was an erratum, which had been the subject of correspondence between the Junior editor and myself. This erratum was the result of transcription. The amount of the several items, was correctly written, but one of the items which in the field-book reads 72° 55′ had been transcribed 72° 05′. The Junior editor discovered the discrepancy between the items and their amount, and very naturally corrected the latter instead of the former, at the same time kindly addressing a note to me on the subject, which, as I was absent, I did not receive. It went to press in the erro-

neous form, and has been used by Prof. Loomis to show the imperfections of my observations in a "striking light." It appears, on page 89, first as $-42'$, second corrected to $-46'.5$, and again, "By far the greater error here, is $-46'.5$, which I obtained from the first observation." Here the whole affair is of no account, because the *datum* on which the calculation was founded, had no real existence. The same observation has been corrected by myself, and was reprinted in the January number for 1841, page 150. This typographical error should have been detected, as such, from its inconsistency with the other items of the same group.

Prof. Loomis assumes in the next paragraph, that the differences of the readings with the face of the compass *east*, and with it *west*, if they exceed a certain constant quantity by him calculated, and called "twice the zero error of the instrument," are "errors of observation," and upon this assumption, makes two tables of errors consisting of 88 items each, at the head of which stands the "*typographical error.*" Here Prof. Loomis has fallen into the prevailing sin of mathematicians, the hasty assumption of *data*, which being granted, they can kill an army "by computation." The proper zero error of the instrument arises from a want of exact adjustment of the spirit level to the zero of the graduated circle, and would be a *constant quantity.* There is also another source of error, arising from want of exact adjustment of the agate planes on which the pivots of the needle are supported; both of which errors are more or less merged by the reversal of the instrument, as they would be plus with the face of the instrument in one position, and minus with it in the opposite position. Now this last error is a *variable* one, being dependent upon the total magnetic force, at any place, which force is *variable.* Probably Prof. Loomis intended by "zero error" the conjoint effect of the relation of both the zero of graduation and of the agate planes to the spirit level. A nice calculator, especially when he is pointing out the errors of other people's labors, should have made a distinction between them, for one is a constant quantity, and the other a variable one. But Prof. L. has treated the whole as a constant. He has also assumed mechanical perfection in the parts of the instrument in contact, the pivots and the agate planes, a condition which never exists in fact. These vacillations in the various reversals which Prof. Loomis has tabulated, were of course known to me, and were always compared during the observations. They were apparent

on the face of the published results and must be apparent by a *coup d'œil* to every professional observer. Why then such a labored table of them? But they all amount to nothing, *literally they amount to nothing;* for in my mode of manipulating, adopted since Sept. 1839, they disappear from the "mean results;" like the zero error they are mostly corrected by the reversals. I could point out what I conceive to be the cause of these vacillations, and the mode in which I have succeeded in merging them; but I have neither time nor an inclination at present, to go into details on these points. I had intended to publish the result of my experience, but the duty to which I am here called, is very far from that of being an instructor of others. I certainly ought not to be forward in volunteering my services in that capacity.

The evidence which convinces me of the correctness of the mean results of my observations, is of the most popular kind; a sort of evidence which every body can understand, equally applicable to moral and to physical snbjects, entirely independent of *hypothesis* and *assumptions* of any kind, and as ancient as the pentateuch. It is simply the agreement of two independent physical witnesses to the same result, under repeated and varied trials. The dip is twice taken, at each place, by a mean of 16 readings of two separate needles. Out of 16 pairs of mean results, at different places, published in the last number of this Journal, there were but two, where the separate needles disagreed more than 1′ of a degree. What is common sense to infer from this? that there is an error of "30 minutes" in any one of them, both the needles taking a fancy, or conspiring, to agree to the same falsehood. Prof. Loomis will not dispute the facts above stated, and we submit the case, whether they do not authorize the conclusion which I have drawn, "that the results are true to within 2 or 3 minutes."

Prof. Loomis has taken no notice of this part of the evidence of the accuracy of the mean results of my observations. What must I infer from this?*

Lastly, Prof. Loomis cites a discrepancy between the dip at Cincinnati, as observed by me in 1837, and the same observed more recently, as proof that my observations are erroneous. I had myself published the fact of that discrepancy, and had it un-

* Should the two needles each have pivots of any other form than that of the cylinder and be alike, and alike placed in reference to the axis of the needle, they might agree in an erroneous indication. But such a concurrence can scarcely happen.

der consideration. Prof. L. will observe that I have attached no uncommon accuracy to my mean results, until the observation at Davenport in Sept. 1839, when I changed my mode of using the instrument. The observation of 1837 was the second one ever made by me, the instruments had imperfections since remedied, and the probability is, that the compass had accidentally been turned out of the meridian, a thing specially guarded against in all subsequent researches.

Prof. Loomis mentions the "disheartening anomalies" which he meets with in his own researches, and makes some remarks concerning his own observations. He will perceive by the rules which I have prescribed to myself, that I can say nothing upon either of those topics.

RECAPITULATION.

1. Prof. Loomis has marked the results of my observations in error, by an assumed hypothesis, *admitted to be such, by himself.*

2. He has thought it "implied" that I did not read both ends of the dipping needle, and that the results were therefore not entitled to so much confidence. *This is not true.*

3. He has found a discrepancy in an item of an observation, at Davenport, as published in Vol. xxxix, of this Journal. *This turns out to be a typographical error.* Prof. L. calculates that the item *ought to have been* 54.1 minutes. The item *actually was* 55 minutes.

4. He has tabulated the small vacillations in the parts of an observation amounting at most to 8′.75 as "errors of observation." *These errors correct each other, and nearly disappear in the mean results.*

ART. III.—*Observations on the Melanians of Lamarck;* by S. S. HALDEMAN.

LAMARCK included the genera Melania, Melanopsis, and Pirena, in this family, without having had it in his power to examine the animals. Férussac unites Pirena to Melanopsis, because he has examined P. atra, Lam. preserved in spirits, and finds that it does not differ from Melanops buccinoidea and costata, which he described from living individuals. Rang takes the same view, and places Melanopsis between Cerithium and Planaxis. Cuvier ad-

mits Melanopsis and Pirena, placing them after Melania and Rissoa; and before Acteon (Tornatella) and Pyramidella. Deshayes (Lam. vol. VIII, p. 488,) appears inclined to unite the three genera, for besides placing part of Pirena in Melanopsis, he unites the remainder to Melania, into which he believes Melanopsis can be merged by the gradual disappearance of its conchological characters. Thus there is a great difference of opinion upon these genera, and much of the confusion can be traced to the fact that the characters of the animal of Melania, instead of being taken from an undoubted species, (as M. subulata or Virginica,) have been drawn from Pirena aurita, Mül.; which, although it approaches Melania in its conchological characters, is generically distinct; so that MM. Deshayes' and Rang's generic characters of the former, apply to the latter alone. Pirena aurita, (figured as Melania aurita, by Rang, on pl. XII of Guerin's Mag., 1832,) is represented with quite a long foot, which is much extended beyond the mouth, and tapering posteriorly; the tentacles are not annulated, the head is scarcely proboscidiform; and the edge of the mantle is fringed—("ayant ses bords découpés."—Desh.) Melania, on the contrary, has the following characters, drawn from M. Virginica.

Animal with a truncated proboscidiform head, bearing two annulated tentacles, upon an enlargement of the outside basal portion of which the eyes are situated, but never beyond the middle of the tentacle; the mouth is provided with a double row of file-like teeth on each side; the foot is oval, not extending beyond the muzzle, slightly thickened, and of medium size; edge of the mantle continuous and simple. The exposed parts are colored with blackish lines upon a yellowish ground, which run transversely across the rostrum and tentacles.* Oviparous.

Thus it will be perceived that this genus has scarcely any essential point in common with those mollusca from the rivers of West Africa, of which P. aurita may be considered the representative. Melania is more like Paludina, but the distinctions are sufficient to place them in different, though adjoining families. An animal with a fringed mantle can scarcely enter the family of Melanians proper, so that P. aurita and its congeners, must be placed in another, of which this is, perhaps, the most tangible character; namely, the CERITHINAE.

I have never seen a living Melanopsis, but as Deshayes places Anculosa prærosa in this genus, and as Férussac's description of the animal applies pretty well to Anculosa, I will take it for

* MELANIA, pl. I, fig. 1—2, of my Monograph of the N. American fluviatile univalves.

granted that the two are nearly allied, and discuss the question of the identity of Melanopsis with Melania. If they are connected, it must be by means of Anculosa, which stands between both. The head, neck, and foot of the Melaniæ, are protruded to a considerable extent; they inhabit rivers, in running water, and are continually moving from place to place, but they are not found in the ripples or more rapid parts of the stream. The Anculosæ, on the other hand, live attached to stones and rocks in the most rapid waters; they are of sedentary habits, seldom moving, except to leave the water occasionally, by climbing up a wet rock. The animal extends but little beyond the shell, as it would be liable to receive injuries from the rapidity of the current; the foot is very small, discoidal in shape, and adapted to enable the animal to hold with great tenacity. They are thus separated from Melania by habit and structure, and a short shell is necessary, to prevent them from being forced from their position by the current; which would of course, have a greater hold upon a long shell. On this account I think it probable that Pirena atra bears the same analogy to the typical Melanopsides, that Melania does to Anculosa; and as these differences could not be well distinguished in dead and contracted specimens, the fact that Férussac referred the former to Melanopsis proper, is of little account. I accordingly adopt this species as the type of the genus Pirena, as Lamarck and Cuvier have done; and retain it among the Melanians, because the mantle is not fringed, but merely sinuated according to the outline of the aperture, as described by Férussac.

"Pirena aurita" is not congeneric with Pirena (atra) nor Melania, but must be placed in the family Cerithinæ; next perhaps to Potamis. The type is distinguished from the typical Melaniæ by the tubercles, and the sinuated labrum; and it cannot be separated from its American representatives, Melania undulata, Say, and similar shells, which, although the characters are less highly developed, have been characterized as distinct by Rafinesque and Swainson. The former author asserts that the animals are distinct from Melania, but the shells of several intermediate species appear to indicate a connection, and if the soft parts present a similar change, there will be some difficulty in pointing out the extent of the genus; and indeed, to separate the Melanians from the Cerithinæ. If 'Pirena aurita' be placed in the former, the distinctions between the two families, must be looked for in the animal of Potamis.

Art. IV.—*Short notices of American Fossil Fishes;* by W. C. Redfield, Mem. Yale Nat. Hist. Soc., &c.

[Received March 15, 1841, and read before the Yale Nat. Hist. Soc. April 28.]

The limited attention which is given by naturalists to the fossil fishes of the United States, is probably owing to the somewhat rare occurrence of these fossils in our rock formations. Hitherto, they have been found chiefly in the red sandstone formations of Massachusetts, Connecticut and New Jersey.

The earliest notices of these fossils appear to have been given by the late Dr. Mitchill, Prof. Hitchcock, and Dr. Dekay. At a later period, some imperfect specimens and drawings of American species received the notice of Prof. Agassiz: to whose invaluable labors this department of science is so greatly indebted. Two species from the Connecticut sandstone have been noticed by my son, Mr. John H. Redfield, in the Annals of the New York Lyceum of Natural History. The existence of fossil fishes in the rocks which overlie the bituminous coal deposit near Richmond, in Virginia, had also been casually noticed, in the American Journal of Science. More recently, it has been made known that these fossils are also found in the red sandstone of New Jersey.*

With the partial exceptions above stated, there appears to have been no attempts to characterize or describe these interesting fossils. I venture, therefore, to notice and designate, provisionally, the several species which, within a few years past, have fallen under my observation.

All the species hitherto found in the above named formations are distinguished, like other ancient fishes, by angular or rhomboidal scales covered with bony enamel: and hence they belong to the order *Ganoides*, in the arrangement of Agassiz; a living type of which is found in the *Esox osseus* or bony pike of our southern and western waters. They are also included in the family *Lepidoides;* and are referable to at least two distinct genera.

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* Remains of one or two species of Holoptychus have been found recently in the *old* red sandstone beneath the coal measures, at Blossburg in Pennsylvania. Mr. Conrad has specified the *H. nobilissimus.* See Am. Jour. of Science, Vol. xxxviii, p. 89.

Genus I. Palaeoniscus, of Agassiz.

In this genus the dorsal fin is found opposite the center of the interval between the ventral and anal fins; and many species have been described by Agassiz, in his great work on fossil fishes. An imperfect specimen from this country he has designated as the *P. fultus;* referring to the stout character of the fins and their insertions. But this character is also found to pertain, in a greater or less degree, to all the known American species of this genus, and would perhaps warrant their separation from the *Palaeonisci.* The raylets or armatures attached to the anterior margins of the several fins are also strong, few in number and of unequal length and inclination. The scales, and apparently, the vertebræ, are prolonged into the upper lobe of the tail; but to a more limited extent, than in the European species of the genus.

1. *Palaeoniscus fultus: Agassiz.*—The specimen figured by Prof. Agassiz, is destitute of the dorsal and head, as well as the upper portion of the body. The length was probably about four and a half inches; but this is often exceeded in other specimens. The fins and their bony insertions appear stouter than in the *P. latus,* but less stout than in some other species.*

Found at Westfield,† Middlefield,† and Durham, in Connecticut; Sunderland, in Massachusetts; and Boonton, in New Jersey.

2. *Palaeoniscus latus: J. H. Redfield.*—Broad Palaeoniscus. The common length of this species is from four to five inches; and its width from one and a half, to two and a quarter inches. It is figured in the Annals of the New York Lyceum of Natural History, Vol. iv.

Found at Westfield, Middlefield, and Durham, Ct., and Boonton, N. J.

3. *Palaeoniscus macropterus: W. C. R.*—Long finned Palaeoniscus. This species is distinguished by the longitudinal extension of the dorsal and anal fins; which thus seem to present a remote resemblance to the wings or forked tail of the common swallow. Its length is commonly from five to seven inches; and its width from one and a half, to two inches.

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* Another specimen shown in the drawings of Prof. Agassiz, was copied from one of the early and imperfect figures published by Prof. Hitchcock.

† Westfield and Middlefield are outlying districts or parishes of the township of Middletown, in Connecticut.

Found at Westfield, Middlefield, and Durham, Ct.; Sunderland, Mass.; and Boonton, N. J.

4. *Palaeoniscus Agassisii: W. C. R.*—Large Palaeoniscus. To this, which is the largest of the American species yet discovered, I propose to affix the name of this distinguished naturalist. Its length, in the specimens hitherto obtained, varies from seven and a half, to ten inches, and its width from three to four inches. The fins, with their armatures and insertions, are also of more remarkable thickness than in the species already noticed. The large scales or plates which belong to the anterior portion of the dorsal line, are commonly found doubled together at their lateral edges, by the incumbent pressure, which gives them the appearance of short spines, or flattened rays; and hence these are sometimes mistaken for an anterior comb-like dorsal.

The same appearance is found, not unfrequently, in some of the smaller species above noticed, and in two of the figures which were published by Prof. Hitchcock, these narrowed and erected scales are made to appear as a fringe-like dorsal fin. Prof. Agassiz has been led to place one of the figures thus drawn, in his genus *Eurynotus*, under the name of *E. tenuiceps*. The specimen thus figured, was probably a *P. latus*, or perhaps belonged to another undescribed species.

5. *Palaeoniscus ovatus: W. C. R.*—Wide, or round-shaped Palaeoniscus. This species is shorter than *P. Agassisii*, and exceeds all the known American species in the comparative width or roundness of its form; and is also remarkable for the large size of its scales. It is of rare occurrence, and owing probably to its great thickness, is seldom obtained in a perfect form. This fossil also exhibits the spine-like erections of the dorsal scales which have been noticed above.

Found at Westfield and Middlefield, Ct.; Sunderland, Mass.; and Boonton, N. J.

Genus II. Catopterus, of J. H. Redfield.*

This genus is characterized by the backward position of the dorsal fin, which is nearest the caudal extremity and opposite to the anal fin. It is also distinguished by the articulated and delicate form of the rays which compose the several fins. The anterior margins of the latter are fringed with numerous small ray-

* Described in the Annals of the New York Lyceum of Nat. Hist. Vol. iv.

lets; which present a finely serrated appearance. The scales and vertebræ appear to extend into the upper lobe of the tail, but in a more limited degree than appears in most of the older fossil fishes of Europe. The following species may be assigned to this genus.

1. *Catopterus gracilis: J. H. R.*—Larger Catopterus. The length of this species varies from near seven to nine and a half inches; its width is from one and a half to near two inches. The dorsal fin is small, nearly triangular, and is placed opposite the center of the broad and flowing anal fin. The pectoral fins are of an elongated form, and are strengthened on the anterior margin by one or two large and partially flattened rays, to the front of which, the fringe of fine raylets is attached. Owing to this peculiarity of structure, the smallest section of the pectoral fin will often serve to identify this species.

Found at Westfield, Middlefield, Durham, and Southbury, Ct.;* Sunderland, Mass.; and Boonton, N. J.

2. *Catopterus macrurus: W. C. R.*—Large finned or Virginia Catopterus. This beautiful species is distinguished by its broad and flowing fins; of which the anal is so extended as to be nearly joined to the caudal fin. The latter is finely extended. The length of this species is from four to five inches; its width from one and one eighth, to one and three eighth inches. The fringes of raylets on the anterior margins of the fins are remarkably fine and beautiful. The posterior margins of the scales seem to be curled slightly outward, giving the surface a somewhat roughened appearance.

Found in Chesterfield county, Virginia; twelve miles from Richmond. Parts of near twenty specimens were found on a single piece of the rock which was presented to the New York Lyceum, the extreme length of which did not exceed twelve inches.

3. *Catopterus anguilliformis: W. C. R.*—Eel shaped Catopterus. This remarkable species, as hitherto found, is from seven to near ten inches in length; width from half to three fourths of an inch. It has a finely forked and extended caudal fin of delicate structure; a well extended dorsal; and all the fins are fring-

* The *Palaconisci* are also found at Southbury, in the red sandstone basin of the Housatonic valley, as appears from some fragments obtained by Mr. J. H. Redfield; but no distinguishable specimens have come under my observation.

ed with the fine raylets which pertain to this genus. The impressions of the fins are, usually, but faintly visible; owing, probably, to their delicate structure. The scales are equally indistinct; and the impression of the head is seldom visible.

Found at Westfield and Middlefield, Ct.; Boonton, N. J.; and, as I have been informed, at Sunderland, Ms.

4. *Catopterus parvulus: W. C. R.*—Little Catopterus. This small and delicate fossil is but obscurely developed in the few specimens which have been obtained. The extremely fine spread caudal and other fins; with their slender frontal raylets; serve to mark it as a member of the genus: although these raylets are fewer in number and of greater and more unequal length than in the other species. In the few specimens obtained, the caudal extremity is commonly found in a bent or half twisted position.

Found at Middlefield, Ct.; Sunderland, Ms.; and Boonton, New Jersey.

In view of the paucity of organic remains in the red sandstone rocks of New England and New Jersey, geologists will appreciate the value of these fossils, as indicating the comparative age or identity of the formations in which they are found. The rocks containing these fossils, also exhibit peculiarities of stratification, dislocation and lithological appearance, as well as a similarity in other small but undetermined fossils, which tend to establish the cotemporaneous character of these formations.

New York, February 3, 1841.

ART. V.—*Contributions to English Lexicography;* by Prof. J. W. GIBBS.

No. II. Account of some American Indian Words found in English.*

Cacao, (probably an American Indian word;) the chocolate-tree, a species of the *Theobroma*, a native of the West Indies.

Cacique, (from the native Mexican and Guatemalan languages through the French; comp. Span. *cacique*, Port. *cacico*, Fr. *cacique*;) the title of the native chiefs in Mexico, Guatemala, and perhaps other parts of America, at the time of the conquest by the Spaniards.

* For No. I. see Vol. xxxiii, p. 324.

Calumet, (thought by Dr. Duponceau to be derived from Fr. *chalumeau*, a reed; but more probably an American Indian word, see the quotation from a French writer below;) an Indian pipe for smoking tobacco, the accepting of which, with proper ceremonies, was a sign or symbol of peace and friendship, and the rejection of it, a symbol of war.

"Ils [les savages] ont comme les autres l'usage de la pipe, qu'ils appellent *calumet*."—*Father G. Marest in Lettr. Edif.* IV. 21.

Cariboo, (probably an American Indian word;) an animal of the stag kind.

Chocolate, (Mexican, *chocolatl*, probably connected with *cacao*, which see;) a paste or cake composed of the kernel of cacao, with other ingredients.

Hackmatack, (thought by Dr. Dwight to be an American Indian word,) the popular name of the red larch, *Pinus microcarpa*.

Hommoc, (thought by Dr. Webster to be an American Indian word;) a hillock or small eminence of a conical form.

Hommony, (Powhatan, *homony*, broken maize;) maize hulled and broken, but coarse, prepared for food by being mixed with water and boiled. Hence a vulgar comparison in many parts of this country "as coarse as hommony."

Inca, or *Ynca*, (Peruvian, *king* or *lord*, comp. Choctaw, *ingka*, father;) an appellation given to a dynasty of Peruvian kings, and to the princes of the blood, before the conquest of that country by the Spaniards, and to the descendants of these kings since.

Maize, (probably an American Indian word;) Indian corn, or the native corn of America, *Zea Mays*.

Moccason, (a word derived from the Massachusetts Indians, but found in most of the languages of the Algonkin-Lenape family; as, Knistinaux, *moscasin;* Algonkin and Chippeway, *makisin* and *maukisin;* Scoffi, *masteshun;* Souriquois, *mekezen;* Abenaki, *mkessen;* Massachusetts, *mohkissonah;* Narraganset, *mocussinass;* Mohican, *mkissin;* Delaware, *maksen;* Nanticoke, *meckissius;* Pamptico, *moggison;* Powhatan, *mockasin;* Miami, *m'kasiu;* Illinois, *mahkissina;* Menomeni, *maukahshen;* some of which have a plural termination;) an Indian shoe or covering for the foot.

Mohawk or *Mohock*, (the native name of one of the Iroquois tribes of Indians;) an Indian of the Mohawk tribe; by extension, a ruffian in the streets of London.

Moose, (a word derived from the Algonkin-Lenape family of languages; as, Narrag. *moosquin*, a fawn; *moose*, the skin of the red deer; Miami, *musuoh* or *mohsokeh*, a deer; Illinois, *mousoah*, a deer;) an animal of the genus *Cervus*.

Mush, (according to Dr. Webster from Germ. *mus*, pap; perhaps a corruption of the Indian word *maize*;) the meal of maize boiled in water.

Netop, (Narraganset, *netop*, a friend;) a friend or crony, said by Dr. Pickering to be used in some of the interior towns in Massachusetts.

"What cheares, nétop, is the general salutation of all the English towards them, [the Indians.]"—*Roger Williams*, 1643.

Papoose, (Massachusetts, *pappouse*; Narrag. *papoos*; Mohican, *papoose*;) an Indian child.

Potato, (Amer. Ind. *batatas*;) a plant and esculent root, of the genus *Convolvulus*, a native of America.

Powwow, (Massachusetts, *powwow*; Narrag. *powwaw*;) an Indian priest, exercising also the offices of physician and conjurer.

Quahaug, (thought by Dr. Webster to be an Amer. Ind. word;) a species of clam.

Sachem, (a word derived from the Massachusetts Indians, but found in many of the languages of the Algonkin-Lenape class; as Knistinaux, *okemow*; Chippeway, *ogima* or *okimau*; Ottawa, *okemah*; Algonkin, *okimaw*; Abenaki, *sangman*; Etchemin, *sockum*; Massachusetts, *sachem* or *sagamore*; Narraganset, *sachim*; Delaware, *sakima*; Shawno, *okema*; Menomeni, *okomow*;) an Indian chief or prince.

Sagamore, (merely another form of the word *sachem*, which see;) according to Dr. Dwight, an inferior sachem, but probably synonymous with sachem.

Sagoin, (probably an American Indian word;) an animal of the genus *Simia*.

Samp, (Massachusetts, *nasampe*; Narraganset, *nasaump*; see the quotation from Roger Williams below;) maize boiled with milk.

"*Nasaump*, a kind of meale pottage, unpartch'd. From this the English call their *samp*, which is the Indian corne, beaten and boild, and eaten hot or cold, with milke or butter, which are mercies beyond the *Natives* plaine water, and which is a dish exceedingly wholesome for the English bodies."—*Roger Williams*, 1643.

Sapajo, (probably an American Indian word;) an animal of the genus *Simia*, found in America.

Squash, (according to Richardson, a pumpkin which is easily *squashed*, but more probably an American Indian word, comp. Narraganset, *askutasquash*, a vine apple; see the quotation below;) a plant of the genus *Cucurbita*.

"*Askutasquash*, a vine apple. Which the English from them [the Indians] call squashes about the bignesse of apples of severall colours, a sweet, light, wholesome, refreshing."—*Roger Williams*, 1643.

Squaw, (a word derived from the Massachusetts Indians, but found in most languages of the Algonkin-Lenape family; as, Knistinaux, *esqui;* Ottowa, *uque;* Algonkin, *ickweh;* Sheshatapoosh, *sehquow;* Abenaki, *naukskoue*, girl; Massachusetts, *squau* or *eshqua*; Narraganset, *squaws;* Mohican, *peesquasoo*, girl; Long Island, *squah;* Delaware, *okhqueh* or *khqueu;* Nanticoke, *acquahique;* Illinois, *ickoe;* Shawno, *equiwa;* Sauki, *kwyokih;*) an Indian woman.

Succotash, (an American Indian word, Webster;) food of green maize and beans boiled together.

Tobacco, (Haytian, *tabaca*, a pipe for smoking;) the name of various species of the *Nicotiana*.

Tomahawk, (an Algonkin-Lenape word; as, Micmac, *tomehagan;* Abenaki, *temahigan;* Massachusetts, *togkunk;* Mohican, *tumnahecan;* Delaware, *tamahicun;*) an Indian hatchet.

Tomato, (thought by J. Thomson to be an American Indian word;) the love-apple, *Solanum lycopersicum*, originally from S. America.

Wampum, (a contraction of Massachusetts, *wampumpeage*, Indian money, thought to be connected with Massachusetts, *wompi*, white, or Iroquois, *wampum*, a marine shell;) shells or strings of shells used by the Indians for money.

Wigwam, (an Algonkin-Lenape word; as, Knistinaux, *waskyegun;* Chippeway, *wikiwam;* Ottawa, *wigwauk;* Algonkin, *wikiwam;* Micmac, *wigwom;* Abenaki, *wigwam;* Massachusetts, *wikwam;* Mohican, *weekuwuhm;* Delaware, *wiquoam;* Miami, *wikameh;* Illinois, *ouitiame;* Shawno, *wiggewoam;* Menomeni, *weekeewaum;*) an Indian hut or cabin.

Yankee, (according to Heckewelder, Indian, *Yengees*, a corruption of the name *English*, which the Indians applied to the people of New-England;) an inhabitant of New-England.

ART. VI.—*Origin of the Names of Beasts, Birds, and Insects;* by Prof. J. W. GIBBS.

THE later investigations in comparative philology, it is thought, enable us to give, with more precision and plausibility than has been usual, the origin of the names of certain animals, and at the same time to throw light on the origin of common nouns generally.

The names of beasts, birds, and insects, are formed

I. By derivation;

1. From the verbal root, by change of vowel or internal inflection merely; as,

Ape, (Anglo-Sax. *apa*, Germ. *affe*, Old Germ. *affo*;) from Old Germ. √ *av* or *af*, to imitate; as if *the imitator*.

Buck, (Germ. *bock*, Old Germ. *pocch*;) from the root of Old Germ. *puhhan*, to thrust, and Eng. *to poke*; as if *the thruster*.

Bull, (Germ. *bull*;) from the root of Germ. *bellen*, to bark, Anglo-Sax. *bellan*, to roar, and Eng. *to bell*, (to cry as a hart,) or *to bawl*; as if *the roarer*.

Chough, (Anglo-Sax. *ceo*, Fr. *choucas* and *chouette*;) from the root of Eng. *to caw* or *to haw*; as if *the cawer* or *hawer*.

Cow, (Sansc. *go*, Germ. *kuh*, Old Germ. *chua*;) from the root of Germ. *kauen*, Old Germ. *chiuwan*, and Eng. *to chew* or *to chaw*; as if *the chewer* or *chawer*, that is, *the ruminator*.

Crab, (Gr. κάραβος, Lat. *carabus*, Anglo-Sax. *crabba*;) from the root of Anglo-Sax. *creopan*, and Eng. *to creep*; as if *the creeper*.

Crow, (Germ. *krähe*, Anglo-Sax. *crawe*;) from the root of Germ. *krähen*, Anglo-Sax. *crowan*, to crow, croak, and Eng. *to crow*; as if *the croaker*.

Duck, from the root of Eng. *to duck*; as if *the plunger*.

Flea, (Lat. *pulex*, Germ. *floh*, Anglo-Sax. *flea*;) from the root of Germ. *fliehen*, Anglo-Sax. *flean*, and Eng. *to flee*; as if *the fleer*.

Fly, (Germ. *fliege*, Anglo-Sax. *fleoga*;) from the root of Germ. *fliegen*, Anglo-Sax. *fleogan*, and Eng. *to fly*; as if *the flyer*.

Fox, (Germ. *fuchs*, Old Germ. *vuhs*;) from Old Germ. √ *vu*, yellow; as if *the yellow-colored*.

Frog, (Anglo-Sax. *froga* or *frocga*, Germ. *frosch*, Old Germ. *vrosc*;) from the root of Eng. *to frisk*; as if *the leaper*.

Hare, Anglo-Sax. *hara*, Germ. *hase*, Sansc. *sasa*;) from Sansc. √ *sas*, to spring; as if *the springer*.

Hen, (Germ. *henne* and Old Germ. *henna*, fem. of Germ. *hahn* and Old Germ. *hano*, a cock;) from Old Germ. ✓ *han*, (=Lat. ✓ *can*,) to sing; as if *the singer*.

Horse, (Old Germ. *hros*, Germ. *ross*;) from Sansc. ✓ *résh*, to neigh; as if *the neigher*.

Hound, (Sansc. *swan*, Gr. κύων, Lat. *canis*, Goth. *hund*;) from the root of Goth. *hinthan*, and Eng. *to hend*, (in *apprehend*,) to seize; as if *the seizer*.

Lop, a flea, (Anglo-Sax. *loppe*;) from the root of Anglo-Sax. *hleapan*, and Eng. *to leap*; as if *the leaper*.

Midge, (Anglo-Sax. *myge*, *mygge*, or *micge*, Germ. *mücke*, Old Germ. *muccha*;) from the root of Old Germ. *muhhon*, to swarm over; as if *the overswarmer*.

Mouse, (Gr. μῦς, Lat. *mus*, Germ. *maus*;) from Sansc. ✓ *mush*, to pilfer; as if *the pilferer*.

Nag, (Old Dutch *negge*;) from the root of Anglo-Sax. *hnægan* and Eng. *to neigh*; as if *the neigher*.

Ox, (Sansc. *ukshan*, Goth. *aulisa*, Old Germ. *ohso*, Germ. *ochs*;) from Sansc. ✓ *vah*, (=Lat. ✓ *veh*, Eng. *wag*,) *to draw* or *carry*; as if *the drawer*.

Ram, (Germ. *ramm*, Anglo-Sax. *ram*;) from the root of Germ. *rammen*, to push, and Eng. *to ram*; as if *the pusher*.

Snake, (Germ. *schnake*, Anglo-Sax. *snaca*,) from the root of Anglo-Sax. *snican*, to creep, and Eng. *to sneak*; as if *the creeper*. Comp. Germ. *schlange*, a snake, from *schlingen*, to wind; as if *the winder*.

Snipe, (Dutch, *snip*, Germ. *schnepfe*, Old Germ. *snepha*;) from Old Germ. ✓ *snap*, (=Dutch and Eng. *snip*;) as if *the nipper*.

Swan, (Germ. *schwan*, Old Germ. *suan*;) from Old Germ. ✓ *sun*, (=Sansc. ✓ *swan* and Lat. ✓ *son*,) to sing; as if *the singer*. There are some swans that *sing*.

Toad, (Anglo-Sax. *tade*, Dan. *tudse*;) from the root of Dan. *tude*, to grumble; as if *the grumbler*.

Wether, (Germ. *widder*, Anglo-Sax. *wether*;) from the root of Germ. *wideren* and Anglo-Sax. *withrian*, to oppose; as if *the butter*.

Whelp, (Anglo-Sax. *hwelp*;) from the root of Anglo-Sax. *gilpan*, to cry out, and Eng. *to yelp*; as if *the yelper*.

Wolf, (Goth. *wulfs*, Germ. *wolf*, Anglo-Sax. *wulf*;) from the root of Goth. *wilwan*, to tear; as if *the tearer*.

2. Other names are formed by derivation from the verbal or nominal root, strengthened or increased by suffixes which have no apparent significancy.

(1.) *M* strengthened by a labial *b* or *p;* as,

Lamb, (Goth. *lamb,* Old Germ. *lamp,* Germ. *lamm;*) from the root of Old Germ. *limmen,* to bleat; as if *the bleater.*

(2.) *L* or *r* strengthened by a palatal *ge* or *ke,* which in English passes into *ow;* as,

Sparrow, (Germ. *sperling,* Old Germ. *sparo,* Upper Germ. *sperk,* Anglo-Sax. *spearwa;*) from the root of Germ. *sparen,* Anglo-Sax. *sparian,* and Eng. *to spare;* as if *the layer up.*

Swallow, (Low Germ. *swaalke,* Anglo-Sax. *swalewe,*) from the root of Anglo-Sax. *swelgan* or *swilgan,* to swallow, and Eng. *to swill;* as if *the swallower.*

(3.) *M* strengthened by *se;* as,

Breeze, (Germ. *brehme* or *bremse,* Old Germ. *premo,* Anglo-Sax. *brimsa* or *briosa;*) from the root of Old Germ. *primman* and Germ. *brummen,* to buzz; as if *the buzzer.*

(4.) The root strengthened by *el;* as,

Snail, (Germ. *schnegel,* Old Germ. *snekil,* Anglo-Sax. *snegl;*) from the root of Old Germ. *snahhan,* Anglo-Sax. *snican,* to creep, and Eng. *to sneak;* as if *the creeper.*

Weasel, (Anglo-Sax. *wesle,* Old Germ. *wisala,* Germ. *wiesel;*) from Old Germ. *wisa* and Germ. *wiese,* a meadow; as if *the meadow animal.*

(5.) The root strengthened by *en;* as,

Raven, (Anglo-Sax. *hræfen, hrefen,* and *ræfen,* Germ. *rabe;*) from the root of Lat. *rapio,* Anglo-Sax. *hreafian* or *reafian,* Germ. *rauben,* and Eng. *to rob;* as if *the pilferer.*

(6.) The root strengthened by *et;* as,

Cricket, (Dutch *kriek* or *krekel,* Fr. *criquet;*) from the root of Eng. *to crick* or *to creak;* as if *the creaker.*

Emmet, or by contraction *ant,* (Germ. *ameise,* Anglo-Sax. *æmet* or *emet;*) from Old Germ. √ *am,* to labor; as if *the laborer.*

Linnet, (Anglo-Sax. *linetwige,* Fr. *linot;*) from the root of Anglo-Sax. *linet,* Eng. *lint,* Fr. *lin,* and Lat. *linum;* as if *the flax-bird.*

3. Other names are formed by derivation from the verbal or nominal root by means of significant suffixes.

(1.) By means of the suffix *ard,* (=Germ. *hart* or Eng. *hard,*) forming ampliatives; as,

Bayard, from Eng. *bay;* as if *the great bay one.*

Buzzard, (Germ. *busshard*, Fr. *busard;*) from Fr. *buse*, losing its ampliative signification.

(2.) By means of the suffix *el*, forming diminutives; as,

Cockerel, from Eng. *cock;* as if *the young cock.*

Hoggerel, from Eng. *hog;* as if *the young hog*, but applied only to a young sheep.

Spaniel, (Fr. *épagneul*, comp. *Espagnol*, Spanish;) from Fr. *Espagne* and Eng. *Spain*, losing its diminutive signification; as if *the Spanish dog.*

(3.) By means of the suffix *en*, forming diminutives; as,

Chicken, (Anglo-Sax. *cicen*, Dutch *kieken* or *kuiken*, Germ. *küchlein;*) from Anglo-Sax. *cocc* and Eng. *cock;* as if *the young cock.*

Kitten, (Fr. *chaton*, Germ. *kätzchen;*) from Fr. *chat*, Germ. *katze*, and Eng. *cat;* as if *the young cat.*

(4.) By means of the suffix *er*, denoting the personal subject; as,

Beaver, (Lat. *fiber*, Fr. *bièvre*, Germ. *biber;*) from the root of Germ. *bauen*, to build; as if *the builder.*

Chafer, (Anglo-Sax. *ceafor;*) from the root of Anglo-Sax. *ceowan* and Eng. *to chew* or *chaw;* as if *the chewer* or *chawer.*

Plover, (Fr. *pluvier;*) from the root of Lat. *pluvia*, rain; as if *the rainbird.*

Skimmer, from Eng. *to skim.*

Spinner, a spider, (Germ. *spinne;*) from the root of Germ. *spinnen* and Eng. *to spin.*

(5.) By means of the suffix *ster*, denoting the subject, whether male or female; as,

Hamster, a species of rat, (Germ. *hamster;*) from Old Germ. √ *ham*, to hide; as if *the hider.*

Lobster, (Anglo-Sax. *loppestre;*) from the root of Anglo-Sax. *hleapan* and Eng. *to leap;* as if *the leaper.*

(6.) By means of the suffix *ling*, denoting the subject; as,

Sanderling, from Eng. *sand;* as if *the sand bird.*

Starling, from Eng. *stare*, losing its peculiar significancy.

(7.) By means of the suffix *th*, forming abstract nouns; as,

Sloth, from Eng. *slow;* as if by metonymy, *the slow one.*

(8.) By means of the suffix *an* or *ant*, (Lat. *anus*, Provençal *á*, fem. *ana*, Ital. and Span. *ano*, Portug. *ano* and *ão*, Fr. *an*, *ain*, *ien*, Walach. *ën*, *an*,) forming gentile nouns; as,

Pheasant, (Gr. φασιανός, Lat. *phasianus*, Fr. *faisan*, fem. *faisande*, Germ. *fasan*;) from the river *Phasis*; as if *the Phasian.*

(9.) By means of the suffix *ent*, (=Lat. *ens*, gen. *entis*,) forming participial adjectives; as,

Serpent, (Sanse. *sarpa*, Gr. ἑρπετόν, Lat. *serpens*;) from Sanse. √ *srip*, (=Gr. √ ἑρπ, Lat. √*serp*;) as if *the creeper.*

(10.) By means of the suffix *on*, (Gr. ων, gen. οντος, Lat. *o*, gen. *onis*;) forming participial adjectives; as,

Dragon, (Gr. δράκων, Lat. *draco*, Germ. *drache*;) from the root of Gr. δέρκω, to see; as if *the sharpsighted.*

Lion, (Gr. λέων, Lat. *leo*, Germ. *löwe*, Old Germ. *lewo* or *hliuwa*, Anglo-Sax. *leo*;) from the root of Old Germ. *liuwon*, Anglo-Sax. *hlewan* or *hlowan*, and Eng. *to low*; as if *the lower.*

(11.) By means of the suffix *on*, denoting the subject; as,

Capon, (Gr. κάπων, Lat. *capo*, Fr. *chapon*, Ital. *cappone*;) from the root of Gr. κόπτω, and Fr. *couper*, to cut or mangle; as if *the mangled.*

Falcon, (Lat. *falco*, Germ. *falke*, Old Germ. *valho*, Fr. *faucon*, Ital. *falcone*;) from the root of Old Germ. *valo*, Germ. *fahl*, Fr. *fauve*, and Eng. *fallow*; as if *the fallow-colored.*

Griffon, (Gr. γρύψ, Lat. *gryps*, Fr. *griffon*, Ital. *griffone*, Germ. *greif*;) from the root of Germ. *greifen* and Eng. *to gripe*; as if *the seizer.*

Pigeon, (Fr. *pigeon*, Ital. *piccione*;) from the root of Ital. *piccare* and Eng. *to peck*; as if *the pecker.* Comp. Fr. *beccasse*, from *bec*, a beak.

Stallion, (Fr. *étalon*, Ital. *stallone*;) from the root of *stall*; as if *the stall horse.*

(12.) By means of the suffix *ock*, forming diminutives; as,

Bullock, (Anglo-Sax. *bulluca*, Germ. *bullochs*;) from Eng. *bull*; as if *the young bull.*

4. Other names are formed by derivation from verbal or nominal roots, by means of prefixes; as,

Antelope, from Gr. ἀντί, equal to or resembling, and ἔλαφος, a stag; as if *stag-like.*

5. Other names are formed by onomatopeia, or derived from the natural sound of the bird or insect.

(1.) Where there is a repetition of the natural sound; as,

Cuckoo, (Sanse. *kókila*, Gr. κόκκυξ, Lat. *cuculus*, Fr. *coucou*, Span. and Port. *cuco*, Germ. *kuckuk*, Dutch *koekkoek*, Old Slav.

gzegzolta, Lithuan. *geguze;*) formed from the sound, with remarkable uniformity, in languages very remote.

Hoopoo, (Gr. ἔποψ, Lat. *upupa*, Fr. *huppe*, Port. *poupa*, Dutch *huppup;*) from the root of Eng. *to whoop;* as if *the whooper*.

Owl, (Lat. *ulula*, Germ. *eule;*) from the root of Lat. *ululo*, Germ. *eulen*, and Eng. *to howl;* as if *the howler*.

(2.) Where there is considerable fancy in the representation of the sound; as,

Katydid, the popular name of an American insect, the *Pterophylla concava* of Say.

Whippoorwill, the name of a bird.

(3.) Where a suffix is added; as,

Turtle, (Lat. *turtur*, Fr. *tourterelle*, Ital. *tortora*, *tortorella*, and *tortola*, Anglo-Sax. *turtle;*) from Lat. *turturilla*, diminutive of *turtur*, and that formed onomatopoetically.

II. The names of beasts, birds, and insects, are also formed by composition.

1. By imperfect composition, the two words denoting distinct attributes of one and the same object, and their syntactical connection being that of the conjunction *and;* as,

Camelopard, (Gr. καμηλοπάρδαλις, Lat. *camelopardalis*, Germ. *kameel-pardel;*) from Gr. κάμηλος, a camel, and πάρδαλις, a panther; as if *combining the attributes of the camel and panther*.

Leopard, (Gr. λεόπαρδος or λεοντόπαρδος, Lat. *leopardus*, Germ. *leopard;*) from Gr. λέων, a lion, and πάρδος, a panther; as if *combining the qualities of the lion and panther*.

2. By perfect composition, the two words forming one complex idea.

(1.) The two parts of the composition being in apposition or concord with each other; as,

Bittour or *bittern*, (Low Lat. *botaurus*, Fr. *butor*, Span. *bitor*, Dutch *butoor;*) from Lat. *bos*, an ox, and *taurus*, a bull; as if *the bull ox*.

Cormorant, (Fr. *cormoran*, Span. *cuervo marino;*) from Lat. *corvus*, a raven, and *marinus*, belonging to the sea; as if *the sea raven*. Comp. Welsh *morvran*, i. e. sea raven, denoting the same bird.

Ostrich, (Gr. στρουθός, Lat. *struthio*, Span. *avestruz*, Port. *abestruz*, Fr. *autruche*, It. *struzzo*, Germ. *strauss;*) from the root of Lat. *avis*, a bird, and of Germ. *strotzen* or Eng. *to strut;* as if *the strutting bird*.

Porcupine, (Ital. *porco-spinoso ;*) from Lat. *porcus*, a hog, and *spinosus*, thorny ; as if *the spinous hog.*

So *blackbird ; bluebird ; grossbeak*, (as if *grossbeaked ;*) *hummingbird ; redbird ; redbreast*, (as if *redbreasted ;*) *redstart*, (as if *redtailed ;*) *redtail*, (as if *redtailed ;*) *wryneck*, (as if *wrynecked.*)

(2.) One part of the compound standing in rection or government in reference to the other ; the latter part of the composition expressing the principal or leading idea, and the former part some modification thereof.

(a) Where the first term expresses the dative case or the case of participation ; as,

Squirrel, (Gr. σκίουρος, Lat. *sciurus*, dimin. *sciuriolus*, Fr. *écureuil ;*) from Gr. σκιά, a shade, and οὐρά, a tail ; as if *having its tail for a shade.*

(b) Where the first term expresses the accusative case or the relation of the object ; as,

Moldwarp, (Germ. *maulwurf*, Old Germ. *mulwerf ;*) from Old Germ. *mul* or Eng. *mold*, and Old Germ. *werfan*, to turn ; as if *the mold-turner.*

Ossifrage or *ospray*, (Lat. *ossifraga*, Fr. *ossifrage* or *orfraie ;*) from Lat. *os*, gen. *ossis*, a bone, and *frango*, to break ; as if *the bone-breaker.*

Oystercatcher, the name of a bird.

Woodpecker, a bird that pecks holes in trees.

(c) Where the first term expresses the modal case or the relation of the mode or manner ; as,

Crossbill, as if *having a bill like a cross.*

Spoonbill, as if *having a bill like a spoon.*

(d) Where the first term expresses the locative case, or the relation of the place where or time in which ; as,

Grasshopper, which needs no explanation. Comp. Germ. *heuschrecke*, from *heu*, grass, and *schrecken*, to spring, the name of the same insect.

May-bug, as if *the bug appearing in May.*

May-fly, as if *the fly appearing in May.*

Nightingale, (Germ. *nachtigall*, Anglo-Sax. *nihtegale ;*) from Germ. *nachti* or Anglo-Sax. *nihte*, in the night, and Germ. *gällen* or Anglo-Sax. *galan*, to sing ; as if *the night singer.*

Woodcock, as if *the cock living in the woods.*

(e) Where the first term expresses the instrumental case, or the relation of the place by or through which; as,

Snowbird, as if *the bird that appears with the snow.*

(f) Where the first term expresses the genitive or adnominal case; as,

Ladybird, *Ladybug*, *Ladycow*, *Ladyfly*; as if *the bird*, *bug*, *cow*, *fly*, *of the Virgin Mary.*

3. By inverted composition, the order of the terms being contrary to that just exhibited.

(1.) In words peculiar to the English language; as,

Cutwater.

Lapwing.

Turnstone.

Wagtail. Comp. Gr. *κίλλουρος*, *σεισοίρα*, *σεισοπυγίς*, Lat. *motacilla*, Low Sax. *wippsterz.*

(2.) In words derived from the French; as,

Chanticleer, from Fr. *chanter*, to crow, and *clair*, clear; as if *the loud crower.*

(3.) In words derived from the Greek; as,

Hippopotamus, (Gr. *ἱπποπόταμος*;) from Gr. *ἵππος*, a horse, and *ποταμός*, a river; as if *the river horse.*

III. The names of animals are often derived from remote languages.

1. From East Indian dialects; as, *zebu.*

2. From Shemitish dialects; as, *ass*, *camel*, *gazelle*, *giraff*, *goat*, *jackal*, *kid*, *scorpion.*

3. From Mongolian dialects; as, *argali*, *yak.*

4. From Malay and Oceanic dialects; as, *babyrousa*, *kangaroo*, *orang-outang*, *wombat.*

5. From African dialects; as, *chimpanzee*, *gnu*, *koba*, *korin*, *zebra*, *zerda.*

6. From American Indian dialects; as, *aia*, *capibar*, *carcajo*, *cariboo*, *moose*, *paca*, *sagoin*, *sapajo*, *tapir*, *tamarinunau*, *wapiti.*

Art. VII.—*Abstract of the Proceedings of the Tenth Meeting of the British Association for the advancement of Science.*

Concluded from Vol. xl, p. 345.

Col. Sykes communicated the contents of a letter from India, from Capt Aston, on the subject of a recent singular *shower of grain*. He stated that 60 or 70 years ago, a fall of fish had occurred during a storm in the Madras Presidency. This fact is recorded by Major Harriott, in his "Struggles through Life," as having taken place while the troops were on the line of march, and some of the fish falling upon the hats of the European troops, they were collected and made into a curry for the general. This fact was probably for fifty years regarded as a traveller's tale, but within the last ten years, so many other instances have been witnessed, and publicly attested, that the story is no longer doubted. The shower of grain above mentioned, took place March 24, 1840, at Rajket in Kattywar, during one of those thunder storms to which that month is subject, and it was found that the grain had not only fallen upon the town, but also upon a considerable extent of country round the town. Capt. A. collected a quantity of the seed, and transmitted it to Col. Sykes. The natives flocked to Capt. A. to ask for his opinion of this phenomenon: for not only did the raining of grain upon them from heaven, excite terror, but the omen was aggravated by the fact that the seed was not one of the cultivated grains of the country, but was entirely unknown to them. The genus and species was not immediately recognized by some botanists to whom it was shown, but it was thought to be either a Spartium, or a Vicia. A similar force to that which elevates fish into the air, no doubt operated on this occasion, and this new fact corroborates the phenomena, the effects of which had been previously witnessed.

The Secretary read two papers from Mr. Rowall, on *Rain*, and on the cause of the *Aurora Borealis*, and *Magnetism*. His hypothesis is, that each particle of vapor in rising through the air carries with it its portion of electricity, according to its expanded surface; that if condensed within the electrical attraction of the earth, the extra quantity of electricity is withdrawn, and the vapor falls and becomes *dew;* but if it rises beyond the electrical attraction of the earth, and is then condensed, the electri-

city, being insulated, forms an atmosphere around each particle of vapor: which surcharge of electricity not only suspends the vapor by its lightness, but also repels the neighboring particles of vapor, and prevents the formation of rain; and on the removal (by any cause,) of the electricity including the vaporous particles, the repulsion is removed, and the particles of vapor then attract each other, and form rain. Another cause of the formation of rain he believes to be the pressure of gravitation: thus if a cloud begins to form, the accumulation of vapor is on every side, but especially from above, and clouds are often seen piled to a great height; now each particle of vapor on forming the cloud, must have its extra charge of electricity over the particles of the cloud instantly dispersed through the whole mass; would become of the same density as the mass, and would take its level according to its density in the atmosphere, if not prevented by the space being occupied, and would therefore press on the vapor below it; and although the repulsion of the particles of vapor be sufficient to prevent the formation of rain at the edges and thinnest parts of the cloud, the pressure at the greatest depths of the cloud may be sufficient to overcome the repulsion and form rain. Concussions, he conceives, such as those of thunder, would aid the process, and cause heavy rain. He entered into proof of these positions in the papers. They account in his opinion, for the fact observed by Prof. Phillips, that more rain is received in gauges near the ground than in those higher up. He conceives a test of this theory may be had by raising conductors to clouds by the aid of balloons, discharging their electricity: and thus he thinks rain might be produced exactly when needed. His views respecting the cause of the Aurora and of magnetism are consequences of his theory of vapor and rain. The particles of vapor most expanded on rising from the earth, would carry with them a greater quantity of electricity, and would be buoyed up by the electricity to a greater height in the air than that which rises in a less expanded state. Thns, in the tropics, through the action of a vertical sun, vapor would rise to a great height with a great accumulation of electricity: this vapor, carried by the superior trade winds toward the poles on each side, there would be a constant circulation of electricity, a continual rising of vapor, especially in the tropics, carrying a great accumulation of electricity to the coldest parts of the earth, where the electricity again es-

capes to the earth, and rushes along its surface, with the vapor in the lower parts of the atmosphere, towards the equator, and is again carried back by rising vapor to the poles in constant succession, interrupted only in part, by the intense cold of the polar regions, causing the air to be then comparatively dry. The least disturbance then, taking place in the highly charged vapor, (either by part of the electricity being drawn off to the earth, or by vapor diffused from the more temperate regions, or by the accession of vapor either more or less charged with electricity,) must cause an instantaneous flash to pass through the whole mass of vapor, by the rush of electricity to restore the equilibrium, thus establishing the Aurora. Magnetism he ascribes to the constant circulation of electricity, and shows that this opinion will account for the leading phenomena, polarity, daily declination, variation, and constant oscillation of the magnetic needle.

Mr. Espy read a paper to show that the four fluctuations of the barometer which occur daily, are produced entirely by the increasing and diminishing elasticity of the air due to increasing and diminishing temperature. When the sun rises, the air begins to expand by heat; this expansion of the air, especially of that near the surface of the earth, lifts the strata of air above, which will produce a reaction, causing the barometer to rise; and the greatest rise of the barometer will take place when the increase of heat in the lower parts of the atmosphere is the most rapid, probably about 9 or 10 A. M. The barometer, from that time, will begin to fall; and at the moment when the air is parting with its heat as fast as it receives it, the barometer will indicate the exact weight of the atmosphere. The barometer, however, will continue to descend on account of the diminished tension of the air, and consequent sinking upon itself, as the evening advances; and its greatest depression will be at the moment of the most rapid diminution of temperature, which will be about 4 or 5 o'clock. At this moment the barometer will indicate a less pressure than the true weight of the atmosphere. The whole upper parts of the atmosphere have now acquired a momentum downwards, which will cause the barometer to rise above the mean, as the motion diminishes, which must have taken place some time in the night. This rise will be small, however, compared with that at 9 or 10 A. M. As the barometer now stands above the mean, it must necessarily descend to the mean at the

moment when it is neither increasing nor diminishing in temperature, which will be a little before sunrise. If this is a true explanation of the four fluctuations of the barometer in a day, it will follow that the morning rise ought to be greater at considerable elevations, provided they are not too great, because some of the air will be lifted above the place of observation; and such was found to be the case by Col. Sykes in India. As this morning rise of the barometer depends on the increasing elasticity of the air, and this increasing elasticity, on heat, Mr. E. proposed to the mathematicians to calculate how much the whole atmosphere is heated from sunrise till the time when the barometer stands highest, the actual rise of the barometer being given. In this way meteorology may assist astronomy. Prof. Forbes doubted the correctness of Mr. E.'s views of the great daily fluctuations of the barometer at elevated stations: for, towards 2 or 3 o'clock the heat being greatest, its effect in lifting up the inferior air to and above the elevated station, should then be greatest, whereas that time of the day was nearer to the time of minimum height of the barometer than of its maximum. After the announcement of the titles of a few other papers, the business of the Section was concluded.

Sect. B. *Chemistry and Mineralogy.*

A paper on *some electrical phenomena*, and on a *supposed new substance*, was presented by Dr. Schönbein. It is well known to electricians that in certain electro-chemical decompositions a peculiar odor is evolved, very analogous to that produced by common electric sparks, or by the working of an ordinary electrical machine in the air. Dr. S. has undertaken a series of experiments in order to ascertain the circumstances under which this electro-chemical odor is evolved, the causes which influence its production, and if possible, the principle to which its appearance is to be attributed. This peculiar odor is evolved at the anode or positive surface, when certain aqueous solutions are decomposed by the passage of a voltaic current. The oxygen gas which is then evolved has a strong and peculiar smell, which is perfectly similar to that which is always perceived when an electrical machine is worked or sparks passed through the air. M. Schönbein has observed that the odor is evolved on the decomposition of water, dilute sulphuric acid, solutions of phosphoric and nitric

acid, potassa, and many oxysalts; dilute sulphuric acid yielding it in the greatest quantity, while no smell whatever was perceived on the decomposition of solutions of hydracids, chlorides, bromides, or iodides, which not only did not evolve it themselves, but by their presence, even in small quantity, prevented its evolution from solutions which would otherwise have produced it abundantly. He found, on collecting the oxygen gas evolved at the anode, from a solution capable of evolving the odor, that the odor might be preserved for some time, by enclosing the gas in well-stopped bottles. From the characters possessed by this oxygen, he was led to consider the odor due to the presence of a minute quantity of a new and hitherto wholly unknown substance, of considerable importance in many natural phenomena, and he has therefore named it from its most evident character, *ozone*. Its properties are briefly as follows: it is evolved only from solutions containing it, by perfectly clean electrodes of platinum or gold; whilst charcoal and the more oxidizable metals are unable to cause its appearance. It can be obtained only from a cold solution, as heat prevents its evolution. When a piece of one of the oxidizable metals, such as zinc, tin, iron, mercury, &c., or a few drops of solution of the protochloride of tin, or protosulphate of iron, are placed in a portion of oxygen impregnated with ozone, that peculiar substance is almost instantaneously absorbed; and the oxygen becomes inodorous. When perfectly clean and dry plates of gold or platinum are immersed in oxygen containing ozone, they acquire a negatively electric state of polarity: silver and copper also become thus electric, but in a far less degree than gold or platinum. The plates thus polarized retain their electric powers in air for a considerable time, but rapidly lose it when plunged into hydrogen gas, in which, if retained a sufficient time, they acquire an opposite state, becoming positively polarized. He then compares these effects with those produced by the odorous matter peculiar to common electric sparks and brushes. When a perfectly clean and dry plate of gold or platinum is exposed to an electric brush issuing from a charged and conducting point, it becomes positively polarized, and the degree of polarity depends on the nature of the point and the time which the plate has been exposed to the influence of the brush issuing from it. He shows that the power is not due to the mere current of electricity escaping from the point, but to

some substance produced or evolved by it; because if the point be moistened, the electricity still continues to be given off as a brush, but the power of polarizing the gold or platinum plates is lost. A plate thus charged, is perfectly similar in its electrical powers to a plate charged or polarized by immersion in oxygen impregnated with ozone. Heat or exposure to hydrogen, which destroys or inverts the electricity of such a plate, exerts a precisely similar action on plates polarized by exposure to the brush; and likewise, if the plates are not perfectly clean and dry, it is equally impossible to charge them, either by exposure to the brush, or by immersion in oxygen containing ozone. He supposes that there exists, both in the air and water, a very minute quantity of an electrolyte or compound substance, which when decomposed by electricity, evolves, as one of its constituents, the peculiar odorous matter called ozone. He observes, that both from its electromotive power, and likewise from its strong affinity for metals, it is evidently similar to chlorine, bromine, and iodine. Its non-appearance when water is decomposed by electrodes of the more oxidizable metals, he attributes to its entering immediately into combination with those metals: and he considers that when the solution is heated, the affinity of the ozone for metals is so much increased, that it is even able to combine with gold and platinum, thus accounting for its disappearance when heated. By this theory, all the phenomena attendant on its evolution may be easily explained, and it hence becomes very interesting to search for traces of this widely diffused substance. M. Schönbein considers that the smell perceived whenever bodies are struck by lightning, is probably due to a small portion of ozone being set free; and relates a recent case within his own observation, of a church struck by lightning, in which the surrounding buildings to a considerable distance were filled with a bluish vapor having a peculiarly pungent odor. Even in this early stage of the inquiry, it will readily be seen that many curious and unexplained phenomena might be accounted for, if the existence of the supposed electrolyte be proved. Mr. S. proposes devoting all his leisure to the prosecution of this inquiry.

Mr. E. Solly proposed the following mode of *bleaching vegetable wax*. The wax must be melted, a small quantity of sulphuric acid is poured in, composed of one part of oil of vitriol to two of water, and then a few crystals of nitrate of soda stirred in, the

whole is then agitated with a wooden stirrer and kept heated. Nitric acid is thus evolved in considerable quantity and purity, from a large surface, and in such a manner that all the acid evolved must necessarily pass through the melted wax. This method answers the purpose very completely; the process is cheap and rapid, and the residuum, being merely a little solution of sulphate of soda, is easily removed. When it is desired to employ chlorine in place of nitric acid as the bleaching agent, the same process may be adopted.

Prof. Gregory read a paper on the *preëxistence of urea in uric acid*. By the action of peroxide of lead on uric acid, Liebig and Wöhler obtained from it oxalic acid, allantoine and urea, and they considered the latter as existing in the uric acid, combined with urile. The author having found that urea, unlike most organic substances, resists the oxidizing agency of permanganate of potash, thought that if urea could be obtained from uric acid by the action of that salt, the argument for its preëxistence would be much strengthened; as, if only the elements of urea were present, the oxidizing agency of the permanganate would most likely prevent its formation. On trying the experiment, a large quantity of urea was obtained, along with oxalic acid, and a new acid probably formed by the oxidation of allantoine. The author further described the acetate of urea, a salt formed in his experiments.

Prof. Gregory then exhibited a new process, communicated by Prof. Liebig, for preparing the very singular and beautiful compound termed *murexide* by Liebig and Wöhler, and *purpurate of ammonia*, by Prout. The process is quite certain, and very productive. It consists in adding a boiling solution of 7 grains of aloxan, and 4 grains of aloxantine in 240 grains of water, to 80 grains of a cold and strong solution of carbonate of ammonia. The mixture instantly acquires a deep purple color, and on cooling, deposits the golden green crystals of murexide.

On the relation of form to chemical composition, by Dr. Schafhaeutl. The object of this paper is to explain the circumstances under which certain modifications of form take place in Graphite, (as also in others generally considered to be elementary,) and to prove their connexion with changes of an entirely chemical nature.

Account of a *new compound of arsenious and sulphuric acids;* by Dr. Schafhaeutl. This was obtained from the escaping smoke of copper calcining furnaces near Swansea, in South Wales. The new compound was another singular instance where an anhydrous crystallized body was deposited under the presence of water only, and was a remarkable proof of the unlimited number of different forms of combination which might be produced even in inorganic substances in contact under varying circumstances. The copper ores smelted in South Wales were chiefly copper pyrites, mixed with iron pyrites, gray copper ore, &c.; in fact, a mixture in which the sulphurets of copper, iron, arsenic, antimony, cobalt, nickel, zinc and tin were invariably found together. The sulphur and arsenic escape from these ores during the calcining process, as sulphurous and arsenious acids, and have been found to destroy all vegetation for miles around the copper works, without affecting animal life in the slightest degree. By bringing the escaping fumes in contact with steam, and forcing it through burning charcoal, or subjecting it only to a great pressure in contact with steam, the new solid compound was deposited on the cool surfaces of the chambers connected with the calcining furnace. It was deposited in beautiful crystallized leaves or tables, perhaps belonging to the same class as Wöhler's dimorphic modification of the crystallization of arsenious acid, the regular form of which belongs to the octahedron. It was found to consist of

Arsenious acid, - - - - -	68.250
Sulphuric acid, - - - - -	27.643
Protoxide of iron, - - - -	3.029
Oxide of copper, - - - -	0.420
Oxide of nickel, - - - -	0.656
	99.998

These crystals attracted moisture from the air with great rapidity and with evolution of heat, corroding animal and vegetable substances as powerfully as concentrated sulphuric acid. One of the remarkable changes during the formation of this compound, was the conversion of sulphurous acid into sulphuric acid, as well as the presence of iron, copper, and nickel, in a deposit from gaseous matter. No other definite compound of arsenic acid with another acid seems to be known, except those with the organic tartaric and paratartaric acids.

Prof. Thomas Thomson communicated an account of the most important chemical manufactures carried on in Glasgow and the neighborhood. 1. *Iron.* The quantity of iron smelted here cannot be much less than 200,000 tons, nearly a fifth part of all the iron smelted in Great Britain. The ore employed, is the carbonate of iron, or clay iron-stone. The rapid increase of iron-smelting has been the consequence of a discovery of Mr. Neilson, manager of the gas works. This is now universally known under the name of the *hot blast.* The air is heated to more than 607° before it enters the furnace, by passing through a range of heated pipes. Under this treatment, coals may be used without previous coking, and instead of seven tons of coals for every ton of cast iron, three tons or even two and a half, will suffice. 2. *Sulphuric acid.* The manufacture of this acid was begun by Dr. Roebuck, at Prestonpans, about 1763, but it is now more than 20 years since his manufactory was abandoned. The sulphuric acid works, at St. Rollox, on the banks of the Monkland canal, were begun about 45 years ago. They were at first upon a very small scale, but they are now probably the largest of the kind in Europe. The size of the leaden chambers was gradually increased, and the substitution of steam for the water formerly placed at the bottom of the chambers, was a vast improvement. The sulphur is burnt over a stove, and an iron cup containing the requisite quantity of saltpetre, mixed with the requisite quantity of sulphuric acid, is placed over the burning sulphur. By this contrivance the sulphur is completely converted into sulphurous acid, and the whole of the nitric acid carried along with it into the leaden chambers. The acid which collects at the bottom of the chambers, has a gravity of 1.75; or it is a compound of one atom anhydrous acid, and two atoms water. This acid is concentrated by heating it in a platinum still, till the second atom of water is driven off. When in full work, the quantity of sulphuric acid made in it, exceeds 300,000 pounds avoirdupois per week. Forty five years ago, it cost eight pence per pound; the present price is under a penny a pound. 3. *Bleaching powder.* One of the great purposes to which sulphuric acid is applied at St. Rollox, is the manufacture of bleaching powder, or chlorite of lime, as it is now called. The requisite mixture of common salt, binoxide of manganese, and sulphuric acid, is put into a leaden still, and the chlorine evolved passes through

leaden tubes into air-tight stone chambers, the bottoms of which are covered with a stratum of slacked lime several inches thick. The lime absorbs the gas as it passes into the chamber, and the process is continued till the absorption is reckoned sufficient. Pure bleaching powder is a compound of

Chloride of calcium, - - - - -	7.
Chlorite of lime, - - - - -	10.
Water, - - - - - - -	3.375
	20.375

Half the lime loses its oxygen and combines with chlorine, constituting chloride of calcium. The oxygen combines with chlorine, which in the state of chlorous acid, combines with the other half of the lime, constituting chlorite of lime. Two atoms of the water were in the slacked lime. The third atom must have come along with the chlorine gas, or been absorbed from the atmosphere. 4. *Carbonate of soda* is obtained from the semi-liquid mass remaining in the still, and consisting partly of the impurities of the manganese, and partly of sulphate of soda, and sulphate of manganese. 5. The manufacture of *alum* is carried on at two establishments, one at the Hurlet, about six miles S. W. by the Paisley canal; another at Campsie, about eight miles off, near Kirkintulloch, on the great canal, and near the foot of the Campsie Hills. The alum is made from the shale, which exists in great abundance in the exhausted coal beds. This shale is a clay mixed with some coal, and with that variety of iron pyrites which undergoes decomposition, and is converted into sulphate of iron, by exposure to the air. The sulphate of iron thus formed, acts slowly on the clay, and finally converts it into sulphate of alumina. The alum-maker washes this altered shale, and obtains a solution of sulphate of iron and sulphate of alumina. The liquor yields an abundant crop of sulphate of iron, which is removed, dried, and sold at a cheap rate. The sulphate of alumina does not crystallize till it is mixed with sulphate of potash or sulphate of ammonia; because ammonia is a double salt composed of three atoms of sulphate of alumina and one atom of sulphate of potash or sulphate of ammonia. 6. At Campsie alum-works, is prepared the *prussiate of potash*, a well known beautiful yellow salt, crystallizing in truncated octahedrons. The manufacture of this salt on the large scale began here. Before

that time it was prepared only in laboratories for scientific purposes, and sold at a high price. Mr. Mackintosh introduced it to the calico-printers, who use it extensively, to produce very beautiful blues and greens. It is prepared by burning the hoofs and horns of cattle with potash, and some iron in iron pots. The hoofs and horns of a hundred head of cattle are consumed in the works every day. After combustion the residue is lixiviated with water, and when the solution is sufficiently concentrated, the prussiate of potash crystallizes. Connected with this manufactory is one of *Prussian blue*. It is made by mixing sulphate of iron, alum, and prussiate of potash, and precipitating the whole by an alkali. The precipitate is at first light blue, but it is washed with new portions of water every day for several weeks. At every washing the color deepens, and when it has acquired the requisite shade, the Prussian blue is allowed to subside, the water is drawn off, and the powder permitted to dry. The color varies according to the proportion of alum employed, and it has the finest color of all, with the coppery lustre which is so much admired, when no alumina whatever is mixed with it. 7. *Bichromate of potash*, a salt very much used by calico-printers, and forming the finest and most indelible yellows, oranges, and greens, is made at the manufactory of Mr. White, at Shawfield, near Rutherglen, about two miles from Glasgow. Its introduction constituted quite an era in calico-printing. It formerly cost a guinea an ounce; it is now sold at two shillings a pound. *Tartaric acid* is also made here. It is largely used by the calico-printers, chiefly to disengage the chlorous acid from bleaching powder, and enable it to destroy the color on particular parts of the cloth, either that these parts may remain white, or that some other color may be superadded. It is obtained from cream of tartar, by throwing down the tartaric acid by means of lime, and afterwards decomposing the tartrate of lime by means of sulphuric acid, and crystallizing the tartaric. At the same manufactory the carbonate of soda is converted by exposure in an atmosphere of carbonic acid, into sesquicarbonate. It is chiefly used by the makers of soda water. Among other manufactures and processes carried on at this place, were mentioned acetic acid from wood; iodine; soap; bleaching of cotton cloth; Turkish red dyeing; calico-printing; glass making; starch making, &c.

Arthur Connell communicated some additional observations on the voltaic decomposition of alcohol; his experiments appearing to him to prove that water exists as such in absolute alcohol.

On the compound or radical called *kakodyl*, by Prof. Bunsen of Marburg. This is a new radical resembling alcohol, in which arsenic replaces the oxygen of that compound. The oxide of kakodyl has so great an affinity for oxygen, that when exposed to the air it immediately inflames. The bodies produced by the combustion are arsenious acid, carbonic acid, and water. By the further oxidation of the oxide of kakodyl, kakodylic acid is produced. The sulphuret of kakodyl is similar in composition to the oxide, and participates in many of its properties. The telluret, selenuret, iodide and bromide of kakodyl were also examined. The danger attending these experiments is very great, and the poisonous effects produced by the inhalation of the vapor, were described as dreadful. Kakodyl is produced from the liquor of Cadet, and is extremely interesting as being a link connecting organic and inorganic chemistry. Prof. B. is engaged in further experiments on this subject, and has already obtained many new combinations.

New method of preparing Morphine and its salts, by Dr. Mohs, of Coblentz. This mode of separating morphine from narcotine, and all other heterogeneous substances, consists in dissolving it in an excess of caustic lime, and precipitating it by muriate of ammonia. This method of precipitation is in principle very similar to the precipitation of alumina, from a solution in caustic potash. The process is as follows: the opium is boiled in water, in which it readily dissolves; the decoction is strained through a linen cloth and the dregs are pressed; this operation of boiling and straining is repeated twice on the same quantity of opium, and the solution of the whole concentrated until its weight is four times that of the opium employed. The concentrated solution is, while still warm, mixed with milk of lime, prepared with a quantity of dry lime equal to the fourth part of the weight of the opium. The mixture is heated till it boils, and is filtered through linen while hot. The filtered liquor has a light brown yellow color. While still hot it is mixed with pulverized sal ammoniac in excess; the lime is saturated with muriatic acid, ammonia is set free, and the morphine is precipitated. When the solution is greatly concentrated, the precipitation is instantaneous,

and is almost equal in volume to half the solution. When the solution is less concentrated, there is at first no precipitation, but as the liquor cools open needles appear, and at a certain point a large mass of precipitate is suddenly formed. The peculiarity of this process is, that it affords a well crystallized and fine product of morphine, without the use of alcohol. This is due to the fact that the ammonia is not added in a free state, but is generated in immediate contact with the substance to be acted upon. The morphine is nearly colorless: by dissolving it in muriatic acid, and crystallizing, we obtain pure muriate of morphine in white crystals. The milk of lime, it is to be observed, must not be added to a boiling hot solution of the crude opium, for then the precipitate adheres to the sides of the vessel, and does not afterwards redissolve perfectly. The liquor containing the morphine should either be cold or only lukewarm, when the milk of lime is added to it. If it is boiling hot, it must be added to the milk of lime, and not *vice versâ*.

Dr. Schäfhaeutl read a paper on a new method of photogenic drawing.—Prof. Graham gave an abstract of Prof. Liebig's new chemical views relative to Agriculture and Physiology.—Prof. Playfair read a statement of Prof. Liebig's new views on the subject of poisons.—Dr. P. gave an account of a new fat acid.—Mr. J. J. Griffin read a paper on a new method of crystallographic notation. He classes the planes of crystals into seven elementary sets, which he calls *forms;* and entered into various details to prove that the occurrence of planes not representable by one or other of these seven forms, is a mathematical impossibility, and that the proposed system of notation is amply sufficient for all the purposes of the chemist and mineralogist, while it has over other systems of crystallography, the advantage of requiring but a small amount of mathematical knowledge.

Prof. F. Penny communicated details and results of his experiments on the action of nitric acid on the chlorates, iodates, and bromates of potassa and soda.—Dr. R. D. Thomson gave his views on the tests for sulphuric acid when thrown upon the person.—Prof. Johnston stated his experiments on the resin of sarcocolla.—Prof. T. Thomson enumerated the minerals found in the neighborhood, among which were nine species of lead ore, and numerous minerals of the zeolite family. Sulphuret of cadmium had been lately discovered along with prehnite, at Bishoptown:

it was rare, and single crystals sold at £10 each.—Prof. Andrew Buchanan related his method of separating, by filtration, the coagulable lymph from liquid human blood.—Dr. Clark, of Aberdeen, stated his mode of detecting minute portions of arsenic.—Mr. McGregor read a paper containing experiments made by him while resident in the Royal Infirmary of Glasgow, which showed that the per centage of carbonic acid thrown off from the lungs is greater in disease than in health.—A paper by Messrs. Redtenbacher and Varrentrapp was read, on the constitution and products of the distillation of fat bodies.—Prof. Bunsen stated his new *mode of estimating nitrogen in organic analysis.* He introduces the substance to be analyzed, after having mixed it with oxide of copper, into a glass tube. A few slips of metallic copper are then added, and the tube is fixed to Dobereiner's apparatus for producing hydrogen. This gas is conducted through it until all the atmospheric air is expelled, giving the tube at the same time, a rotary motion, in order to dislodge any air which might be retained between the particles of the oxide of copper. The tube is now hermetically sealed, and introduced into an iron vessel, filled with moist gypsum. Thus prepared it is introduced into the common oven used for organic analysis, and surrounded with red-hot coals. If the tube is of strong green glass, it never bursts. When the combustion is completed, the tube is placed below a graduated glass receiver standing over mercury, and the point cut off. The gas, which had a pressure of several atmospheres, now rushes into the jar. The carbonic acid is absorbed by a ball of hydrated potash, which is introduced into it, and the remaining gas must be nitrogen, for all the hydrogen must have been converted into water by the oxygen of the oxide of copper. The results obtained by this method agree with theory to the second and often to the third decimal place.—Prof. Penny gave an account of a new salt obtained from iodine and caustic soda, which he names the sesquiodide of iodate of soda.—Prof. Johnston read a paper on *resins*, containing the following results. 1. The resins differ from each other in the quantity of oxygen they contain. 2. Those in which the atoms of oxygen is the same, may vary in the hydrogen, which is another cause of difference in properties. 3. In all the resins hitherto analyzed, the number of atoms of carbon is constant. 4. The resins, as a natural family, may be represented by a general formula containing two variables. 5. The

known resins divide themselves into two groups, possessing unlike chemical and physical properties: of one, colophony is the type, and of the other gamboge, or dragon's blood.—Prof. Johnston exhibited some varieties of peat from the moss near Paisley, which he said illustrated a transition from the comparatively fresh vegetable matter to a substance resembling coal, but which he affirmed to be ulmic acid. He stated that the same substance might be obtained from peat by digesting it in ammonia, and afterwards precipitating the brown solution by an acid; while, on the other hand, caustic potash extracts another acid, which he proposed to term *humic* acid.—Mr. Alex. Bryson stated a new method of ascertaining the *refractive powers of minute bodies*, and its application to mineralogy. In ascertaining the refractive powers of crystalline substances, it is commonly thought necessary to procure pieces not less than a quarter of an inch in size, which are then to be ground into prisms. The microscope, with a slight alteration, is well suited to give minute differences in refractive powers. On the stage of the microscope is placed a piece of crown glass, with fine lines drawn on its first surface. If a piece of beryl, or any other mineral with parallel sides, is now placed on the glass, the lines will no longer be visible through the microscope, until it is raised above the crystal three hundredths of an inch. The difference of focus becomes an index of the difference of refractive power between the glass plate and the crystal. The means adopted to ascertain minute changes in focal length, is a scale of hundredths of an inch, with a vernier dividing it into thousandth parts.—Prof. Gregory communicated a paper on the preparation of *alloxan*, *alloxantine*, *thionurate of ammonia*, *uramile*, and *murexide*. To prepare alloxan from uric acid, Liebig and Wöhler used nitric acid of sp. gr. 1.42, and separated the acid liquid from the crystals by means of a porous brick, thus losing the whole of the mother liquid. The author uses nitric acid of sp. gr. 1.35. The action of this acid on uric acid must be kept moderate. When crystals of alloxan are formed, the whole is thrown on a filter, the throat of which is stopped with asbestus. That portion of the acid liquid which remains in the crystals is displaced by a few drops of cold water, and the crystals are purified by recrystallization. The liquid is again employed in the same way and the crystals collected as before. Five such operations may be performed with the same liquid, each yielding a large crop of

crystals: while the mother liquid is preserved, and yields a large quantity of parabanic acid, or oxalurate of ammonia. By this process the author obtains from 100 parts of uric acid, 65 of anhydrous alloxan, or 90 of alloxan + 6 aq. From alloxan, alloxantine is easily obtained by the action of sulphuretted hydrogen. Thionurate of ammonia is easily formed, by boiling a solution of alloxan with sulphite of ammonia, and free ammonia. Uramile is also easily obtained, by boiling a solution of thionurate of ammonia, with an excess of dilute sulphuric acid. Murexide is obtained, as has been already described, (p. 46 of this No.) The theory of the formation of murexide is of great importance in reference to organic coloring matters.

Sect. C. *Geology and Physical Geography.*

Dr. Robb presented a communication on the *geology of the country around the River St. John*, in New Brunswick. The St. John is as large as any river of the first class in Europe. It drains a large portion of the province of New Brunswick, and the volume of water which it discharges into the Bay of Fundy is very great, especially during the spring floods. Yet in one place the river is so contracted that it is not more than 310 feet in width. In the Bay of Fundy, as is well known, the tides rise higher than in almost any other part of the globe, there being sometimes, in spring tides, a rise of from 40 to 60 feet. The tidal wave is then forced up into the narrow parts of the river, and causes a backward fall of water, against the natural current, of many feet in height. Dr. R. alluded to the singular configuration of the country in the vicinity of the river, it being shaped in *terraces*, one below the other. The river, from the distance, between the first or uppermost pair of terraces, appears to have been of much greater width, and subsequently to have gradually become contracted, until confined within its present narrow limits. The higher terraces slope towards the stream, the lower ones become more horizontal, and the lowest is with a slope turned from the river; an appearance which Dr. R. explained by the successive depositions of alluvial matter near the bed of the stream, and extending no farther, so that depressions resulted behind these depositions, which often contain water. In no part of the world can the phenomena of rivers be studied better than in America, from the little alteration produced upon them

by the hand of man. In many parts of the new continent the rivers present these terraces; they may be observed near the St. Lawrence, the Mississippi, and in many other places; and had the rivers of the Old World been examined before their banks became cultivated, in all probability they would, in many cases, have displayed similar terraces. Dr R. adverted to the theory which explains the formation of these terraces by the bursting of the barriers of lakes through which the river had passed. He conceived that in a few cases, the phenomena might be so explained, but that in general we must have recourse for the solution of the problem to an upheaving of the land, and that at a comparatively recent period. In these terraces, but few organic remains had been discovered, which he accounted for by the paucity of the limestone rocks from which mollusca could obtain matter for the formation of shells; the long cold winter, too, might have had its influence. He had, however, found in the lower terraces, *Uniones* and *Anodontes* resembling those now existing in the river. Some bones had been discovered, but they seemed to be recent, probably belonging to the spermaceti whale. In the different terraces there is a difference in the quality of the soil, there being most alluvium on the lowest terraces. The middle terraces, being so nearly horizontal, are well fitted for roads, and are used accordingly. At Frederickton, where wells had been sunk, the vegetable soil was three inches deep, after which 14 feet of sand was gone through, when water was reached, retained by a bed of clay, underlaid by a slate rock. The terraces were composed wholly of detrital matter, the upper ones being coarser, and often having boulder stones, some of which were 15 feet in diameter, and seemingly derived from parent rocks to the N. W., inferred from scratches on the ground, coinciding with the major axes of the boulders. The rocks in the neighborhood of the river, are slates, with some limestone, the whole disturbed by sienite.

Prof. Johnston submitted an account of the first part of his *Report on Chemical Geology*. He referred to the combination of science requisite to promote geology. It had drawn upon the labor of the zoologist, the comparative anatomist, the botanist, the historian, the natural philosopher, and had now called in the assistance of the chemist. Prof. J. referred to what had been done by the late Sir Humphry Davy, and the late Dr.

Turner, in their application of chemistry to geology; and he had been requested by the Association to draw up a report of this application. He now brought forward his investigations on the most important of our mineral productions,—*coal.* Although some geologists may entertain a different opinion, he assumes for granted, the vegetable origin of coal; and although it may be classified in various ways for economic or geological convenience, as into caking or not caking, bituminous or not bituminous, the true basis of the classification must depend on the chemical composition. Carbon, oxygen, and hydrogen, are the components of living vegetables, and the same elements compose coal, but in different proportions. In the decomposition of vegetable matter there are two agents always at hand, viz. air and water, which resolve it into carbon, oxygen, and hydrogen, forming with one another these combinations; carburetted hydrogen, carbonic acid, and water. In the change from lignin to fossil wood, we find that carbonic acid is parted with; and this continues without variation in all the kinds down to cannel coal. In mines of lignite and cannel coal we find only carbonic acid, (or choke-damp;) while in mines of coal lower in the scale, we find in addition, carburetted hydrogen, (or fire damp;) the hydrogen diminishing in each variety as we approach the anthracite. In regard to the question whether the vegetable matter that formed coal had been drifted or generated on the spot, he was inclined to the latter opinion.

Mr. Mathie Hamilton presented "Observations on *great earthquakes on the West coast of South America*, particularly the great one of Sept. 18, 1833, which destroyed the city of Tacna, and other places in Peru." Tacna, an Indian town of some antiquity, now capital of the province of the same name, lies in the midst of a desert tract of about 50 miles broad, between the mountains and the sea. The port of Arica, about 40 miles distant, had, since the first arrival of the Spaniards, been five times destroyed by earthquakes, while Tacna had enjoyed a happy immunity, and was supposed beyond the reach of this calamitous visitation. After 1826, however, very frequent and severe shocks were felt, particularly a few weeks previous to the great one of Oct. 8, 1831, which reduced Arica to a heap of rubbish; yet it continued nearly uninjured till the evening of Sept. 16, 1833, when there occurred a single loud report, with an upward move-

ment of the ground. On the morning of the 18th there was a much more violent movement, the earth heaving at once up and down, and also laterally, accompanied by a frightful subterranean noise. The falling of houses all around, the cries of the people, the howlings of animals, produced a scene that cannot be described. The agitation seemed to have reached the utmost possible height, when suddenly the earth, as if striving to get rid of some mighty load, made a movement more terrible than ever, in every direction, and in one minute the work of destruction was completed. The cathedral in falling, destroyed a numerous congregation of females who had assembled there, and were endeavoring to escape; but the priests, who remained under an arch, were saved. It is remarkable that while some quarters had nearly every house demolished, others were comparatively uninjured. Rain, (here a rare phenomenon,) fell almost every day during six weeks; and at Arica, on the first week of October, there came down a deluge, such as had not been witnessed for half a century. The river which supplies Tacna with water, remained undisturbed, but others were changed in their courses, and one altogether disappeared. The earthquake was felt many hundred miles to the south, as far as the desert of Atacama. At Suto, about 40 miles distant, fissures were made in the ground, whence issued a dark colored fluid. In the province of Tarapaca, villages were overthrown; and one, which stood in a ravine, was buried with all its inhabitants. To the north its ravages were equally extensive. The villages of Samo, distant 30 miles, and of Coquimbo, distant 60, were both destroyed. Moquehua, 120 miles off, suffered severe damage; and Arequipa was violently shaken, but with little injury. The effects extended even to the lofty peaks of Upper Peru. Tacora, 15,000 feet above the sea, had its church thrown down. When the atmosphere cleared after the calamity, that mighty range, as seen from Tacna, presented in many parts, a new outline. Large masses had been detached or slid down into the valleys or ravines, leaving many elevated peaks denuded of their most prominent features. Mr. Scott, engineer, then employed at Achozumio, about 14,500 feet high, describes the shocks there as terrific, and the noise as if an immense mass of porcelain had, after being raised in the air, been then let fall, and dashed to pieces. By his telescope, he saw the masses falling from the mountains, one of them leaving a space as large as St. Enoch's

Square, Glasgow. On the 20th of January, 1834, a terrible earthquake occurred in New Grenada, by which the large towns of Popayan and Pasto, were entirely demolished, and many thousands perished. On the 21st of September, 1834, Mr. Hamilton experienced a most severe shock, in which the movements of the earth were entirely vertical, and seemed to take place twice every second. He mentions also, the terrible earthquake on the coast of Chili, February 5, 1835, by which the seaport of Conception and Talcahuano, the capital of the province, were totally destroyed. The sea then retired several times to a great distance, and returned in immense billows. It is believed that new banks were then thrown up from its bottom, and that it was on one of these, that the *Challenger*, ship of war, was wrecked a few months after.

M. Agassiz made a communication on the subject of *glaciers and boulders in Switzerland.* He particularly drew attention to facts relative to the mode of the movements of the glaciers, which he attributes to the continual introduction of water into all their minutest fissures, which, in freezing, constantly expands the mass. The effects of the movement produced by this expansion, upon the rocks beneath the ice, are very remarkable. The bases of the glaciers, and the sides of the valleys which contain them, are always polished and scratched. The fragments of the rocks that fall upon the glaciers are accumulated in longitudinal ridges on the sides of the ice, by the effects of the unequal movement of its middle and lateral masses. The result is longitudinal deposits of stony detritus, which are called *morains;* and as the glaciers are continually pressed forwards, and often in hot summers melted back at their lower extremity, it results that the polished surfaces, occasioned by friction on the bottom and sides, are left uncovered, and that the morains, or curvilinear ridges of gravel, remain upon the rocks formerly covered by the ice, so that we can discover by the polished surfaces and the morains, the extent to which the glaciers have heretofore existed, much beyond the limits they now occupy in the Alpine valleys. It even appears to result from the *facts* mentioned by Prof. A., that enormous masses of ice have, at a former period, covered the great valley of Switzerland, together with the whole chain of the Jura, the sides of which, facing the Alps, are also polished, and interspersed with angular erratic rocks, resembling the boulders in the morains; but so far different, that

the masses of ice, not being there confined between two sides of a valley, their movements were in some respects different,—the boulders not being connected in continuous ridges, but dispersed singly over the Jura at different levels. Prof. A. conceives that at a certain epoch, all the North of Europe, and also the North of Asia and America, were covered with a mass of ice, in which the elephants and other mammalia found in the frozen mud and gravel of the arctic regions, were imbedded at the time of their destruction. He thinks that when this immense mass of ice began quickly to melt, the currents of water that resulted, transported and deposited the masses of irregular rounded boulders and gravel which fill the bottoms of the valleys; innumerable boulders having at the same time been transported together with mud and gravel, upon the masses of the glaciers then set afloat. Prof. A. announced that these facts are explained at length in the work which he has just published, 'Etudes sur les Glaciers de la Suisse,' illustrated by plates.

Mr. Jeffreys detailed an experiment he had made on a very great scale, to decide the question, whether silicious matter could be dissolved largely by water, or what is the same thing, by its vapor. This experiment formed the subject of a paper, read some months ago before the Royal Society, and by the experiment a solution of more than 200 lbs. of silica was effected in steam, at a heat exceeding that of fused cast-iron. The steam was not under pressure, but was conducted into a large kiln used for stone pottery. The silica was not only dissolved, but carried away in the vapor, and some pounds weight of it were deposited from the vapor, before it issued from the kiln, like a hoar frost, upon some articles in the kiln, where the temperature was not above a red heat.

The following papers were also read, but our limits permit us to give only their titles.

On the relative level of land and sea, and on the alteration of the east coast of England; by Mr. Stevenson.

On the superficial beds in the neighborhood of Glasgow; by Mr. Smith of Jordan Hill.

On the Geology of Canada; by Capt. Baddeley.

On the Silurian rocks of Llangollen, and on a plateau of igneous rocks on the east flank of the Berwyn range; by Mr. Bowman.

On ancient sea cliffs and needles in the chalk of the valley of the Seine, in Normandy; by Charles Lyell.

On the geology of the island of Arran; by A. C. Ramsay.

On the geology of Castle Hill, Ardrossan; by Wm. Keir.

On the value of topographical maps and models, with a map of the county of Mayo, in Ireland; by Mr. Bald.

Additional Notes on the Wadi el 'Arabah, in Syria; by Rev. E. Robinson, of New York.

Some observations on relief maps; by Mr. A. Ravenstein.

On the stratified deposits which constitute the Northern and Central regions of Russia; by Mr. Murchison and M. E. de Verneuil.

On the coal formations of the West of Scotland; by Mr. Craig.

On earthquakes in Scotland; by Mr. Milne.

On the occurrence of two species of Shells of the genus Conus, in the lias or inferior oolite, near Caen, in Normandy; by Charles Lyell.

On the yellow sandstone of the carboniferous limestone series of Ireland; by Mr. Griffith.

On a Pleistocene tract in the Isle of Man, and the relations of its Fauna to that of the neighboring sea; by Mr. Edward Forbes.

On the old red sandstone of the Northern Counties of Scotland; by Mr. Murchison.

On the geology of Ceará, North Brazil; by J. E. Bowman.

On the Vale of Solway and Closeburn Basin; by Mr. Knipe.

Sect. D. *Zoology and Botany.*

A paper was read, entitled "Queries respecting the Human Race, to be addressed to travellers and others, drawn up by a Committee of the British Association for the Advancement of Science, appointed in 1839." At the meeting at Birmingham, Dr. Prichard read a paper on *the extinction of some varieties of the Human Race.* He pointed out instances in which this extinction had already to a great extent taken place, and showed that many races now existing are likely, at no distant period, to be annihilated. Science must sustain an irretrievable loss, if so large a portion of the human race, counting by tribes instead of individuals, is suffered to perish before many interesting questions of a psychological, physiological and philological character, as well as many historical facts in relation to them, have been investigated. The Association voted £5 to be expended in printing a set of queries to be addressed to those who may travel or reside in parts of the globe inhabited by declining races. The paper now presented was the result. The subjects embrace a wide field of inquiry, and the queries alone fill thirteen closely printed octavo pages. They refer specially to the stature and weight of the people,—any prevailing proportion between different parts of the body—the complexion—the color and character of the hair and eyes—the formation of the head and face—the skull and all physical peculiarities—the effect of intermarriage where it prevails—health, longevity, physical and intellectual character—language—ceremonies—superstition—education—dress—treatment of sick

—nature of sickness—inferior animals associated with man—ceremonies connected with marriages, births and burials—notions of a future state—habitations of the people—monuments—remains of skeletons—tools and instruments—form of government—food—mode of cooking—clans or castes—laws—geographical limits and character of the region—population—religious observances.

A memoir was read on the *Pollen and Vegetable impregnation;* by Dr. Aldridge, of Dublin. The author having discovered that nitric and other inorganic and organic acids produce the dehiscence of pollen-grains, in the same manner as if placed on the natural stigmatic surface, instituted experiments, of which the following are the general results. 1. The spore of cryptogamic vegetables, which some botanists consider analogous to pollen, do not dehisce under the influence of acids. 2. The pollen of the grasses is spherical, both when dry and when placed in water; with acid it bursts, protruding one long, cylindrical mass, which remains afterward unacted upon by the liquid. 3. The pollen of the Aroideæ, Colchicaceæ, Smilaceæ, Liliaceæ, Commelinaceæ, Bulomaceæ, Amaryllidaceæ, Iridaceæ and Conaceæ, are, when dry, oval, and marked with a dark neutral line; but become, when placed in water, more broadly oval or circular, the long diameter remaining the same, and the opake line disappearing, after the addition of acid; the external membrane of the pollen or peripollen dehisces by a chink or suture sufficiently broad to permit the contents or endopollen to escape without any alteration in its form, after which the endopollen remains unacted upon by the liquid. 4. In the Salicineæ, Salicariæ, Leguminosæ, Rosaceæ, Crassulaceæ, Saxifragaceæ, Hypericaceæ, Rutaceæ, Hippocastaneæ, Resedaceæ, and the tribe Helleboreæ of the Ranunculaceæ, the pollen when dry, oval and marked with a dark central line, becomes when placed in water, round or nearly so, the dark line disappearing; and when acted upon by acids, assumes a triangular form, and protrudes at three equidistant points cylindrical or club-shaped masses very similar at the origin to tubes, and presenting the appearance of being enveloped by a membrane. 5. In the greater number of the remaining Dicotyledons examined, the dry pollen is opake, and either broadly oval or spherical. 6. In the Ericaceæ and Epacridaceæ, the pollen grains when dry, appear triangular or oval in some instances, triangular or rhombic in others, according to the position in which

they are examined. Having ascertained the results of acids on the pollen, the author was induced to examine the stigma, and in every case found that the stigmatic tissue gave indications of an acid reaction upon litmus paper. The next question is, in what manner is the fertilizing influence of the male organs communicated to the ovale? After quoting the opinions of Amici, Brown, Fritzche, Corda, Treviranus, Brongniart, and others, the author came to the conclusion that the *boyaux* or intestine-like protrusion from the pollen-grains is the result of the action of acids upon the fluid which contained the fovilla in the pollen-grain; and he inferred this from the fact that this tube or *boyau* is never found when the pollen-grain is placed in water, but is constant when the grain is placed in acid. After describing and explaining the anomalous character of the pollen-grains in Orchidaceæ, Fumariaceæ, Asclepias, &c. the author gave the following conclusions as the results of his researches. 1. The stigma is invariably acid. 2. It is in consequence of this acidity that the pollen bursts. 3. By the same means the fluid contents of the pollen become coagulated, enveloping the fovilla, and assuming, according to the method of dehiscence, different and very remarkable forms.—The memoir was illustrated by an extensive series of drawings.

A paper on the *Alpaca* was read by W. Danson. Since the meeting at Birmingham, about twenty of these animals had been imported into Liverpool. The alpaca is remarkable for its extraordinarily long wool, the staples being from twenty to twenty four inches in length. The wool is naturally free from grease, in which respect it differs materially from the sheep, attributable to its not perspiring through the skin, and consequently not requiring the artificial protection of smearing with tar and other substances injurious to the wool, as far as the manufacture is concerned; and in shearing, the animal requires no washing preparatory to the operation. The alpaca is hardy, flourishing under the line of perpetual snow, in the mountains of the Andes, (Peru,) and has a peculiar coat of silky wool, which proves a complete protection against an atmosphere at all times excessively humid, and against the deluging rain which continues to fall more than four months in the year. The animal is not only capable of undergoing great fatigue, but moreover lives on mountain herbage, little better than a kind of withered grass, and in times of scar-

city has been sustained several days without water, taking only a handful of maize. Their flesh is considered equal to venison, being commonly eaten by the Peruvians, who state the slaughter of them for food to be about four millions annually. The importation of the alpaca wool in 1839, is stated at one million pounds, and within the last year at three millions. It seemed, however, doubtful whether the animal could be made to thrive in Great Britain.

Prof. Agassiz made a communication on *animals found in Red Snow*. He stated that Shuttleworth had lately demonstrated that besides the *Protococcus nivalis*, the red snow contained several species of infusoria. The results of Prof. A.'s observations led him to conclude that the red snow was altogether an animal production, and that the so-called *Protococcus nivalis* is the ova of a species of rotiferous animal called by Ehrenberg *Philodina roseola*. This animalcule he had found dead in the red snow, and abundantly in ditches in the neighborhood, at the bottom of which its ova produced a red deposit. Under the microscope the colored ova in the ovaries could be distinctly observed. He had also seen the infusoria described by Shuttleworth.

The following papers were also communicated to this Section.

On the preservation of animal and vegetable substances; by Prof. Henslow and the committee.

Further researches on the British Ciliograda; by Edward Forbes and John Goodsir.

On the plants and animals found in the sulphureous springs of Askern and Harrowgate, Yorkshire; by Dr. Lankester.

On the structure of fishes; by Dr. Macdonald.

On the regeneration of lost organs discharging the functions of the head and viscera, by the Holothuria and Amphitrite, two marine animals; by Sir John G. Dalyell.

Report of a committee appointed to dredge various parts of the Coasts of Great Britain for marine animals.

On an anomalous form of the plum observed in the gardens of the province of New Brunswick; by Prof. Robb.

On the identity of the fish called the *parr* with the fry of the salmon; by Messrs Shaw and Wilson.

On the true method of discovering the natural system in Zoology and Botany; by Hugh Strickland.

On the organs of sense in the Salmon; by Dr. Lizars.

On a new Salmon-stair, with a model; by Mr. Smith, of Deanston.

On the development of the embryo in the ova of fishes, more especially of the family Salmonidæ; by Prof. Agassiz.

On a new genus of Ascidians, viz. *Pelonaia;* by Messrs. Forbes and Goodsir.

On the Medusæ; by Mr. Patterson.

On the various modes of fishing employed by Indians in the West of Guiana; by Mr. Schomburgk.

Report on the Irish Fauna, including only the Vertebrata; by Mr. Thomson.

Report of the committee for engraving skeleton maps, for recording the distribution of plants and animals.

On the migration of birds on certain parts of the coast of Ireland; by Sir Thos. Phillips.

Remarks on the synonyms and affinities of some South African genera of Plants; by Dr. W. Arnott.

Further remarks on the bones of a whale found in 1839 in the keep of Durham Castle; by G. T. Fox. The letter dated 1661, which appeared to clear up this fact, proves to be a fabrication. (See Vol. xxxviii, p. 130.)

On the growth of Cotton; by Dr. Lankester and Mr. Felkin.

On India Cotton; by Dr. Alexander Burn.

On the first changes in the ovum in mammiferous animals; by Dr. Martin Barry.

Sect. E. *Medical Science.*

The following communications were presented to the Section.

Report of the London Committee on the motions and sounds of the heart.

On the use of the conglobate glands; by Dr. Jeffrey.

On the diffusion of contagious fevers, the laws which govern them, and the mode by which they are communicated; by Dr. J. Perry.

On the properties, chemical and therapeutic, of the Matias Bark; by Dr. McKay.

On the therapeutic effects of Croton oil in certain affections of the nerves, by Dr. Newbigging.

On the results of amputations; by Dr. Lawrie.

On the medicinal action of bromine and its compounds; by Dr. R. M. Glover.

On opacity of the cornea, produced by sulphuric acid; by Dr. R. D. Thomson.

On dislocations of the ankle-joint backwards and forwards; by Mr. Douglass.

On the physiology of the lungs and bronchi; by Dr. C. J. B. Williams.

Investigation of the physiology of the organ of hearing; by Dr. Sym.

On the manner in which vital actions become suspended in Asphyxia; by Dr. John Reid.

On the subserviency of the fifth pair of nerves to the adjustments of the organs of sense, and of a retransmission through its nasal branch and the lenticular ganglion to the iris; by Dr. R. Fowler.

Notice of Bonten d'Alleppe and Bagdad boil in the East; by Dr. Charles W. Bell.

On the effects of air when injected into the veins; by Dr. J. R. Cormack.

On the anatomy of the Medulla Oblongata; by Dr. John Reid.

On the connexion between the nervous system and muscular contractility; by Dr. John Reid.

On the structure of the Gastro-intestinal mucous membrane; by Prof. Allen Thomson.

On local inflammatory action, the effusion of coagulable lymph, formation of pus, &c.; by Dr. Perry.

On Pertussis; by Dr. Hannay.

On Mnemonics; by Dr. McDonald.

Sect. F. *Statistics.*

The following communications were presented to the Section.

Report on the state of crime within Glasgow and city police jurisdiction, with observations of a remedial nature; by Capt. Millar.

On the population, trade and commerce of the city of Glasgow; by Dr. Cleland.

On the Glasgow Asylum for the blind ; by Mr. Alston.

On the application of Statistics to moral and economic science ; by Dr. Chalmers.

Illustrations of the practical operation of the Scottish system of the management of the poor ; by Dr. Alison.

On the comparative vital statistics of Edinburgh and Glasgow ; by Mr. Watt.

On the bill circulation of Great Britain ; by Mr. Leatham.

On the excess of population, and on emigration as a remedy for it in the Highlands of Scotland ; by Prof. Ramsay.

On the vital statistics of Glasgow ; by Dr. Cowan.

On the state of education and crime in England and Wales ; by Joseph Bentley.

On Pawnbroking in Ireland ; by Mr. Porter.

Tables containing a comparative view of the state of crime in London, Dublin and Glasgow.

On the state of crime in the borough of Calton ; by Mr. Rutherglen.

On the state of crime in the district of Gorbals ; by Mr. Richardson.

On the state of crime in the suburban burgh of Anderston ; by Mr. Findlater.

Report of the Manchester Statistical Society on the state of education in the borough of Kingston-upon-Hull.

On the population of certain parts of Africa ; by Mr. Saxe Bannister.

Sect. G. *Mechanical Science.*

Mr. Wallace offered a paper on *extinguishing fire in steam vessels.* This he proposes to effect by steam itself. The plan has been some time before the public, and many successful experiments have been made in the presence of scientific men. Among the most important was the following, made on board the *Leven* steamboat :—On the cabin floor, a space of 10 feet by 14 was covered with wet sand, on which were laid iron plates, and on these a fire was kindled with about 4½ cwt. of very combustible materials, such as tar barrels, &c. A hose 34 feet long and 2½ inches in diameter, extended from the boiler of the engine to the cabin, and when the fire had been sufficiently kindled, so that the panes of glass in the windows of the cabin began to crack by the heat, the steam was let in, and the doors of the cabin shut. The fire was extinguished in about four minutes. Several trials were made, and all with like success. On another trial, a metal pipe of a greater diameter than the hose was connected with the steam-boiler, and extended into the cabin. A small square hatch was cut in the deck, immediately above the cabin, and through this opening were lowered down into the cabin two movable grates, each containing a blazing fire, well kindled, of about 1 cwt. of coals. The hatch on the deck, and the cabin doors were then shut, and the steam let in, and in fifteen minutes the small hatch was opened, and one of the grates hoisted up, when the whole mass of coal and cinders, which had

before formed a powerful fire, was found to be completely extinguished. This experiment was twice repeated, with equal success.

On the application of *Native Alloy for Compass pivots*, by Capt. E. J. Johnson, R. N.

Among those portions of a ship's compass which most affect its working, are the pivots and caps on which the needle and card traverse, and which like the balance of a chronometer (but of far more importance to the practical navigator) should not only be fitted with the most scrupulous attention to accuracy, but be made of materials capable of maintaining a given form under the trials to which such instruments are necessarily exposed. Having examined a great variety of compasses which had been used at sea, wherein Capt. J. noticed that the pivots were generally injured, and often by rust, he searched numerous records of experiments for its prevention, and for improving the quality of steel in other respects, by means of alloys of platinum, palladium, silver, &c., (alluding particularly to the experiments of Dr. Faraday and Mr. Stoddart,) and Mr. Pepys having obligingly supplied Capt. J. with specimens of similar kinds of steel to those used by them, these examples, together with pivots made of the ordinary kind of steel, and hardened and tempered in the manner recommended by eminent instrument makers, were placed in a frame for experiment; and to these again Capt J. added certain contrivances of his own, such as rubbing a steel pivot with sal-ammoniac, then dipping it into zinc in a state of fusion, and afterwards changing the extreme point. Some specimens he coated with a mixture of powdered zinc, oil of tar, and turpentine; and others again were set in zinc pillars having small zinc caps, through which the extreme point of the pivot protruded, after the manner of black lead through pencil tubes. The whole of the specimens were then placed in a cellar, occasionally exposed to the open air, examined from time to time during more than half a year, and their several states, as respected oxidation, duly registered. The general result was, that not any of the kinds of steel pivots used in this trial, except such as were coated with zinc, remained free from rust, while the pivot made of the *native alloy* which is found with platinum, completely retained its brilliancy. Capt. J. then applied to this singular substance a more severe test, first by placing sulphuric acid, and then nitro-muriatic acid upon it; but even under this trial he could not observe that any change had been effected, although the blade of a penknife subjected to

a similar process, was rusted to the centre. Having enumerated the facts respecting the trials to which he had subjected this curious material, Capt. J. stated the conclusion that he had come to, viz. that it is sufficiently tough not to break, and hard enough not to bend, under the trials to which it would be *fairly* exposed: and that being alike free from magnetic properties and liability to oxidation from exposure to the air, it possesses the qualities requisite for the pivot of the mariner's compass: and he anticipated that when fitted with a ruby cap to correspond, it would be found greatly to improve the working. Besides the use of this substance for compass pivots, Capt. J. stated that it might probably be found advantageous for other instruments, and especially for the points of the axes of the dipping needles, fitted on Mr. Fox's plan, for use on shipboard.—Mr. Hawkins has used this *native alloy* for several years in tipping the points of pens, and not a single instance exists in which any of these pens shows the least symptom of wear. He tried native alloy on a cap, in comparison with ruby, when he found that in the same circumstances, the ruby was ground away with diamond dust twice as rapidly as the native alloy. He had made engravers' tools of the same metal, and when made too sharp they cannot be blunted on the Turkey stone, but only by diamond dust.

The following papers were also read.

On safety-valves for steam-boilers; by Mr. Galline.

On wheels of Locomotive engines; by Mr. Grime.

On the temperature of most effective condensation in steam vessels; by J. Scott Russell.

On warming and ventilating buildings; by Mr. Ritchie.

On timber bridges of a large size, in special reference to Railways; by Mr. Vignoles.

Experimental inquiry into the strength of iron, with respect to its application as a substitute for wood in ship-building; by Mr. Fairbairn.

On raising water from low lands; by Mr. Fairbairn.

Additional notice concerning the most economical and effective proportion of engine power to the tonnage of the hull in steam vessels, and more especially in those designed for long voyages; by J. Scott Russell.

On the action of air and water on iron; by Mr. Mallet.

On the turbine water-wheel; by Prof. Gordon.

On producing true planes or surfaces on metals; by Mr. Jos. Whitworth.

Report of the Committee on the best form of Vessels.

On the economy of railways in respect of gradients; by Mr. Vignoles.

On the fan-blast as applied to furnaces; by Mr. Fairbairn.

On new rain-gauges; by Jas. Johnston, and also by Mr. Thom.

On a Revolving Balance; by Mr. Lothian.

On the combustion of coal and the prevention of the generation of smoke in furnaces; by Mr. Williams.

ART. VIII.—*Remarks relating to the Tornado which visited New Brunswick, in the State of New Jersey, June* 19, 1835, *with a Plan and Schedule of the Prostrations observed on a section of its track;* by W. C. REDFIELD, Esq.*

[From the Lond. Phil. Mag. and Jour. of Science. Revised by the Author.]

IN a paper printed in the American Journal of Science, in which I referred to the support given by Prof. Bache to Mr. Espy's theory of storms, at the meeting of the British Association in 1838, founded upon observations made on the New Brunswick tornado, I have stated, that in my own examinations I had observed numerous facts which appear to demonstrate the *whirling* character of this tornado, as well as the *inward* tendency of the whirling vortex at the surface of the ground; and further, that the direction of rotation was *towards the left*, as in the North Atlantic hurricanes.† It was due to Prof. Bache that my observations should be brought forward; a task which has been too long delayed, partly from a desire that he would revise his former conclusions. The facts now presented form part of the evidence to which I then alluded.

If the effects which I present for consideration be due to "a moving column of rarefied air without any whirling motion at or near the surface of the ground," as maintained by Professor Bache,‡ we might expect to find a relative uniformity in the effects on the two opposite sides or margins of the track. How far this is the case may be seen by inspecting the observations which are found upon the annexed plan of prostrations.

The occurrence of these tornadoes appears to have been noticed from the earliest antiquity; and their violence has been considered as the effect of an active whirling motion in the body of the tornado; this peculiarity of action having often been supported by the testimony of eye-witnesses.

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* [Communicated by Sir John F. W. Herschel, Bart.] This paper was intended by its author to have been read at the late meeting of the British Association in Glasgow, but was unfortunately detained.—J. F. W. H.

† Amer. Jour. of Science, Oct. 1838, vol. xxxv, pp. 206, 207.

‡ Transactions of Amer. Phil. Society, vol. v, p. 417, New Series.

The whirling motion, however, has not been recognized by Prof. Bache, Mr. Espy,* or Prof. Walter R. Johnson,† in their several accounts of the New Brunswick tornado; these writers having been led to adopt or favor a theory of ascending columns in the atmosphere, founded on the supposed influence of calorific expansion accompanying the condensation of vapor.

It is remarkable that previous to this period the evidences of the rotation or other characteristic action of tornadoes appear not to have been duly examined and recorded, nor even to have received the distinct consideration of scientific observers. We are therefore left to seek out the peculiarities of their action, by examining the direction of the prostrations and other effects of the wind; and from a careful induction from the effects which are thus registered as by the finger of the tornado, we may hope to arrive at satisfactory conclusions.

If the numerous prostrations of trees and other objects, which may be observed in the path of a tornado, be the effects of a violent whirlwind, it appears most reasonable to infer that this whirl had the common properties which may be observed in all narrow and violent vortices, viz. *a spirally descending and involuted motion* of the exterior and lower portions of the vortex, rapidly quickened in its gyrations as it approaches toward the centre or axis of the whirl, and thence continued (in the case of the whirlwind) spirally upward, but gradually expanding in its spiral course by an evolute motion in ascending towards the extreme height of the revolving mass.

If we now contemplate the action of this whirling body, while in a state of rapid progression, on the several objects found in distinct portions of its path, we may expect to witness effects of much complexity, particularly as regards direction; and, also, that amid this apparent complexity, some clue may be obtained that will serve to indicate or establish the true character of its action. Some of the effects which may be expected or observed, will be here considered.

1. We may expect to find, in the path of the whirlwind, strong evidence of the inward or vorticular course of the wind at the earth's surface; the violence of which inward motion is clearly

* Trans. Amer. Phil. Society, vol. v, New Series.

† Journ. Academy Nat. Sciences of Philadelphia, vol. vii, part ii.

indicated by the force with which various objects, often of much weight, are carried spirally upward about the axis of the revolving body.

Now the effects of this inward vorticular motion at the surface of the ground, are clearly manifested in the cases before us; and are also well illustrated by Prof. Bache, in his paper on this tornado, although referred by him to a different action.*

2. As the effects which may be observed at various points in the track were produced at different moments of time, and by forces acting in different directions, as well as of various intensities, we may expect to find great diversities in the several directions of the fallen trees and other prostrated bodies; and further, as all the forces, in addition to their inward tendency, have likewise a common tendency in the direction pursued by the tornado, we may expect to find, also, full evidence of this progressive force in the direction of the fallen bodies.

These effects, I need hardly state, are distinctly observed in the case before us; and appear likewise from the observations of Prof. Bache. The results already noticed have been observed also in the tracks of other tornadoes: so that a general inclination, both inward and onward, amid the various and confused directions of the fallen bodies, is distinctly recognized by all parties to this inquiry.

3. It has been often noticed, that where two fallen trees are found lying across each other, the uppermost or last fallen points most nearly to the course pursued by the tornado.

In view of the facts above stated, much pains have been taken to establish, by induction, a central and non-whirling course in the wind of the tornado; first inward and then upward, like that resulting from a common fire in the open air. I do not propose to notice the insuperable difficulties which appear to attend this hypothesis. It is important to state, however, that all the above mentioned effects, when theoretically considered, are, at least, equally consistent with the involute whirling action of an advancing vortex. This important consideration I have not seen recognized by the advocates of the non-whirling theory; and it seems proper, therefore, to point out, as we proceed, other and more distinguishing effects of the whirling action.

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* Transactions of American Philosophical Society, vol. v, N. S.

4. It has been noticed, also, that the directions given to broken limbs and other bodies, by the successive changes in the direction of the wind as the tornado passed over, have been found in opposite courses of change, on the two opposite sides of the track.

This fact, too, has been strongly urged as disproving a rotary motion. But, unfortunately for the objection, this effect accords fully with the rotary action of a progressive mass of atmosphere; as is well known to all who clearly understand the *theory* of rotary storms.

In all such whirling masses the successive changes in the direction of the wind, *result solely from their progressive motion*, and necessarily take place in opposite directions or courses of change on the two opposite sides of the advancing axis. This indication fails, therefore, as a theoretic test; and I now proceed to notice others, which are peculiar to a progressive whirling action.

5. In considering further the effects of such action, we may expect to find that the greatly increased activity of gyration which is always observed near the centre of a vortex, will be indicated by a more violent and irregular action in and near the path pursued by the axis of the whirlwind, than is found under its more outward portions.

This effect is often strikingly exhibited in the path of tornadoes; while, in the supposed ascent of a non-whirling column, it would seem that no part of the surface would be so much exempted from its action, and particularly from its power of prostration, as that lying near its centre.

6. As the effect of rotation must be to produce, on one side of the advancing axis, a reverse motion which is contrary to the course of the tornado, it is evident that on this side the prostrating power will be much lessened; that the cases of prostration, therefore, will be here less numerous; and that some of these, at least, will be produced in a backward direction, more or less opposite to the course of the tornado. By this criterion, not only the whirling movement, but the direction of the rotation also, may be clearly ascertained.

This effect is best observed by comparing the two opposite margins of the track, and is strongly exemplified in the case before us. Here we find, that most of the trees prostrated within

five chains (110 yards) from the northern or left-hand margin of the track, lie in directions which are more or less backward from the course of the tornado. The prostrations in this part of the track are also for the most part less general than on the opposite side of the axis,* a greater portion of the trees being left standing.

It sometimes happens, owing perhaps to the inward or involute motion having exceeded the progressive motion at a particular point, that some inclination backward will be found in the prostrations on the progressive side of the whirl, as seen on the sketch, Nos. 77 to 80. But these unfrequent cases by no means compare with the numerous backward and sometimes *outward* prostrations, found on the reverse side of the whirl, as illustrated by Nos. 1, 3, 4, 7, 9, 10, 12, 13, &c. on the left side of the track. Thus we find here a satisfactory indication that this tornado was a whirlwind; and that the course of its rotation was to the *left* in front.

7. It is also apparent, that the prostrating power of a whirlwind on the side of its reverse motion as just considered, will be limited to a shorter distance than on the opposite or progressive side of its axis.

This is seen in the more limited *extent* of the prostrations on the north or left margin of the track, as compared with the extent of those which incline inward on the right side of the apparent axis. There were many trees standing beyond the northern border of the track, but none had fallen.

8. It follows, in like manner, that on that side of a whirlwind in which the rotary motion coincides with the progressive movement, the prostrating power will not only be increased in its intensity, but will also be effective over a wider space; and that few, if any, of the prostrated bodies will be found to have been thrown backward.

In the case before us, as may be seen in the sketch, the prostrations are found to extend on the southern or right side of the apparent axis to a distance nearly twice as great as on the left side. The same general result has also been noticed in the tracks of other tornadoes which I have examined.

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* There was a vacant space in the belt of wood, immediately to the right of the line *c c* or axis of the tornado, owing to which the effect mentioned does not appear so obvious in the figure.

The facts here considered are too important to be overlooked, and seem fully to establish both the whirling action and the course of rotation.

9. If a rotative action be exhibited, the mean directions of all the prostrations, on each of the two opposite sides, will differ greatly in their respective inclinations to the line of progress, and the mean direction of those on the reverse side will be found more backward than on the opposite side, where the rotative course coincides with the progressive action.

In the case before us, the mean direction of all the prostrations on the right side of the track is found to incline 52 degrees inward from the line of progress. The course of the tornado is here taken to be east; although for the last half mile its course had been a little north of east. On the left side, the mean direction is found to be S. 3° W., or 93 degrees inward and backward; a difference in the mean inclination from the course on the two sides of 41 degrees.*

If we now take the indications afforded by the two exterior portions of the track, to the width of five chains on each side, where the effects are more distinctive in their character, we find on the right side a mean inward inclination of 46 degrees, the mean direction being N. 44° E.; while on the left side of the track the mean inclination is not only inward but 48 degrees backward, the mean direction on this side being S. 48° W. We have thus a mean difference in the inclination of the fallen trees, on the two exterior portions of the track, of no less than 92 degrees.

These indications seem conclusive, also, in favor of the whirling action in the direction from right to left.

10. Although of less importance, it should be mentioned that the diminished action of the tornado which is commonly observed on the hillsides and summits over which it passes, and the greatly increased action in the bottoms of the valleys, and even in deep ravines, afford a strong argument against ascribing the effects to the ascent of a non-whirling rarefied column; as the latter, it would seem, must act with greater force on the hillsides and summits than in the bottoms of valleys. The general correctness of the observation above stated cannot justly be questioned.

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* The inclinations of the fallen trees from the course, on both sides the axis, are reckoned inward and backward.

11. The sudden and extraordinary diminution of the atmospheric pressure which is said to take place at the points successively passed over by a tornado, causing the doors and windows of buildings to burst outwards, seems to afford strong confirmation of a violent whirling motion; for an effect of this kind is necessarily due to the centrifugal and upward force of the vorticular action in the interior portion of the whirlwind. There are no other means known by which such an abstraction of pressure can be effected in the open air. An increase of calorific elasticity, if such were produced, either generally or locally, would not greatly disturb the equilibrium of pressure, being resisted by the surrounding and incumbent weight of the entire atmosphere. Besides, the immediate effect of such increased elasticity might rather be to burst *inward* the windows and doors of buildings exposed to its action.

Some of the more important indications mentioned above appear also from an examination of Prof. Bache's observations; although the latter are not definitely located by him, as regards the extreme borders of the track. Thus, in Fig. 7 of Professor Bache's paper, assuming the course of the tornado to be east, and rejecting a few observations near the centre, to avoid error, we find in twenty observations on the right side of the track, a mean inward inclination of 64 degrees; and for nine observations on the left side, a mean inclination, reckoned inward and backward from the course, of 104 degrees, being 14 degrees backward.

It is stated by Prof. Bache, "that the trees lying perpendicular to the track of the storm, are not those furthest from the centre of that track." This generalization accords with my own observations; but can hardly be reconciled with an inward non-whirling motion in the tornado.

It may appear to some, that in the case of a whirlwind the greater portion of the prostrations on the reverse side of the axis should be found in a backward direction; and so they would undoubtedly be found, were it not for the inward and the progressive action. But the force is here so far lessened by the reverse action above noticed, that in most cases only a small portion of the trees exposed will be thus prostrated; while the greatest force of the whirlwind, on this side, is felt near its last or closing portion and towards the apparent axis, where the inward, together

with the rotative and progressive forces, seem to combine their influence in the closing rush towards the heart of the receding vortex. This appears to account for the nearly opposite directions of prostration found on this side, and it is apparently by this more violent closing action, that many trees which were first overthrown in a direction nearly across the centre of the path, were again moved from their position, or swept onward nearly in the course of the tornado. It is proper to remark here, that an attentive examination of these effects has served to convince me that on the right and more central portions of the track the prostrations for the most part take place either at the outset or under the middle portions of the whirlwind; while on the left or reverse side, up to the line of the apparent axis, and even across the latter, they occur chiefly under the closing action of the whirl, as above described. The violent effects of this central and closing action are more clearly seen as we advance from the left-hand margin towards the centre or apparent axis of the path.

From the causes to which I have just alluded, the effects are usually more violent on and near the line passed over by the axis, than in other portions of the track. This line of greatest violence is found to coincide nearly with the line which separates the inwardly inclined prostrations of the two opposite sides of the track.* The latter line or apparent axis of the track is sometimes called the line of convergence, and is indicated on the figure by the line and arrow *c c*. Along this line, from the causes just mentioned, aided also by the elevating forces about the axis, many of the trees are swept onward, and left with their tops in a direction nearly parallel to the course of the tornado; forming an apparent, but not a just exception, to the more lateral direction which pertains to most of the trees prostrated by the onset of the whirlwind, near the central portions of the track. Indeed, the central or closing violence of the advancing whirl is here so great, that the trees are not unfrequently torn out of the ground and carried onward to considerable distances.

It is proper to state here, that in the tracks of all the tornadoes which I have had opportunity to examine, and in some, at least,

* The line of greatest violence, for the most part, is found somewhat to the right of the line of convergence.

of those examined by others, the course of rotation has been found the same as in the case before us.*

In order to make a just and satisfactory examination of the effects of a tornado, it appears necessary to select portions of the track where the extension of wood or single trees, on each side, is found sufficient to mark clearly the exterior limits of the prostrating power, and where the effects on both sides of the axis are also clearly developed. Our next care should be to ascertain, as near as may be practicable, the line which separates the opposite convergence of the two sides, noticed above as the axis or line of convergence. We should then determine the general direction of this line and of the track at the place examined; which being done, we may proceed to measure the distance to which the prostrations are extended on each side, and then carefully to take the position and direction of prostration of each and of all of the fallen bodies, noting with care, also, any other phenomena which may serve to aid our inquiries. We may thus obtain valuable materials for future analysis; and this course of investigation, if faithfully pursued, will, it is believed, remove all reasonable doubt of the rotative action of these tornadoes. An examination of their probable origin, and the causes of their enduring activity and violence, belongs not to the present occasion.

New York, February 5, 1841.

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* As in the tornado which passed through Allegany county, New York, July 25th, 1838; described by Mr. Gaylord in the American Journal of Science, Vol. xxxvii, p. 92.

SCHEDULE OF THE PROSTRATIONS

Observed on a Section of the Track of the New-Brunswick Tornado, of June 19th, 1835.

No.	Direction of Prostration.	Inclination.
	TABLE I. Left side of the track to the line *b b*—5 chains.	Inward and backward.
a	Tree lies S. 20° W. . .	110°
b	.. .. S. 80 W. . .	170
1	.. .. N. 67 W. . .	203
2	.. .. S.	90
3	.. .. W.	180
4	.. .. S. 80 W. . .	170
5	.. .. S. 40 W. . .	130
6	.. .. S. 40 W. . .	130
7	.. .. S. 80 W. . .	170
8	.. .. S. 10 E. . .	80
9	.. .. S. 50 W. . .	140
10	.. .. S. 50 W. . .	140
11	.. .. S. 26 W. . .	116
12	.. .. S. 50 W. . .	140
13	.. .. S. 65 W. . .	155
14	.. .. South . . .	90
	Mean direction, 16 cases, S. 48° W. Mean inclination from course, inward and backward, 138 degrees.	
	TABLE II. Left side of the axis, from the line *b b* to *c c*—1½ chains.	
15	Tree lies S. 2° E. . .	88
16	.. .. S. 12 W. . .	102
17	.. .. S 35 E. . .	55
18	.. .. S. 62 E. . .	28
19	.. .. S. 25 E. . .	65
20	.. .. N. 80 W. . .	190
23	.. .. S. 20 E. . .	70
24	.. .. S. 80 E. . .	10
25	.. .. S. 45 E. . .	45
26	.. .. S. 10 E. . .	80
27	.. .. S. 45 E. . .	45
28	.. .. S. 20 E. . .	70
29	.. .. S. 60 E. . .	30
30	.. .. S 60 E. . .	30
31	.. .. East. . . .	00
32	.. .. S. 75 E. . .	15
33	.. .. S. 56 E. [Included as belonging by its inclination to this table.]	34
35	.. .. East . . .	00
	Mean direction, 18 cases, S. 37° E. Mean inclination inward, 53 deg.	
	TABLE III. Right side of apparent axis from line *c c* to W E—3 chains.	
21	Tree lies N. 56° E. . .	34
22	.. .. N. 60 E. . .	30
34	.. .. N. 80 E. [Included as belonging by its inclination to this table.]	10
36	.. .. N. 85 E. . .	5
37	.. .. East (two) . .	0
..	.. .. East . . .	0
38	.. .. East . . .	0
39	.. .. N. 30 E. . .	60
40	.. .. N. 70 E. . .	20
41	.. .. N. 55 E. . .	35
42	.. .. N. 50 E. . .	40
43	.. .. N. 78 E. . .	12
44	.. .. N. 45 E. . .	45
45	.. .. N. 45 E. . .	45
46	.. .. N. 25 E. . .	65
47	.. .. N. 35 E. . .	55
48	.. .. N. 40 E. . .	50
	Mean direction, 17 cases, N. 60° E. Mean inclination inward, 30 deg.	

No.	Direction of Prostration.	Inclination.
	TABLE IV. Right side of axis, from line W E to line *a a*—4½ chains.	Inward and backward.
49	Tree lies N. 67° E. . .	23°
50	.. .. N. 45 E. . .	45
51	.. .. N. 22 E. . .	68
52	.. .. N. 3 E. . .	87
53	.. .. N. 30 E. . .	60
54	.. .. N. 10 E. . .	80
55	.. .. N. 35 E. . .	55
56	.. .. North . . .	90
57	.. .. N. 10 E. . .	80
58	.. .. N. 3 E. . .	87
59	.. .. N. 45 E. . .	45
60	.. .. N. 10 E. . .	80
61	.. .. N. 35 E. . .	55
62	.. .. N. 60 E. . .	30
64	.. .. N. 40 E. . .	50
65	.. .. N. 20 E. . .	70
66	.. .. N. 10 E. . .	80
67	.. .. N. 20 E. . .	70
68	.. .. N. 40 E. . .	50
69	.. .. N. 70 E. . .	20
70	.. .. N. 50 E. . .	40
71	.. .. N. 35 E. . .	55
72	.. .. N. 30 E. . .	60
73	.. .. N. 50 E. . .	40
74	.. .. North (two) .	90
75	.. .. North " .	90
76	.. .. North " .	90
77	.. .. N. 20 W. (clump of 3)	110
78	.. .. N. 35 W. . .	125
79	.. .. N. 30 W. . .	120
80	.. .. N. 10 W. . .	100
81	.. .. N. 65 E. . .	25
82	.. .. N. 70 E. . .	20
	Mean direction, 33 cases, N. 24° E. Mean inclination, 66 degrees.	
	TABLE V. Right side of track, from line *a a* to outward limit of prostration—5 ch.	
63	Tree lies N. 65° E. . .	25
83	.. .. N. 45 E. . .	45
84	.. .. N. 40 E. . .	50
e	.. .. N. 55 E. . .	35
85	.. .. N. 70 E. . .	20
86	.. .. N. 23 E. . .	67
87	.. .. N. 31 E. . .	59
88	.. .. N. 20 E. . .	70
89	.. .. N. 22 E. . .	68
90	.. .. N. 10 E. . .	80
91	.. .. N. 55 E. . .	35
92	.. .. N. 70 E. . .	20
93	.. .. N. 55 E. . .	35
94	.. .. N. 68 E. . .	22
95	.. .. N. 25 E. . .	65
	Mean direction, 15 cases, N. 44° E. Mean inclination inward from course of tornado, 46°.	

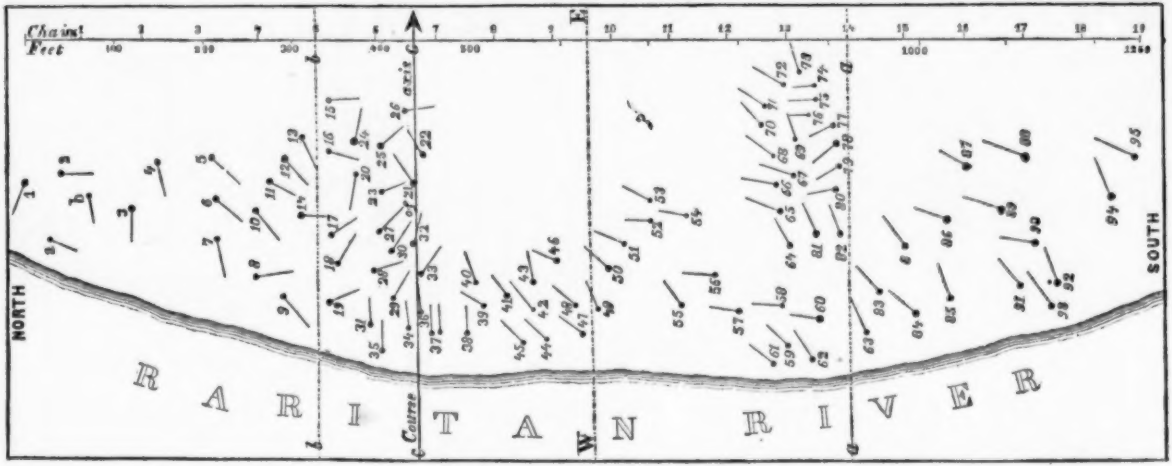
Mean direction of all the prostrations on *left* of axis, 34 cases, S. 3° W.—being 3° backward, or 93° inward and backward.

Mean direction on *right* of axis, 65 cases, N. 38° E.—being 52° inward from course.

Difference of mean inclination on the two sides, 41 degrees.

Difference of opposite marginal sections (Tables I and V) 92 degrees.

NEW-BRUNSWICK TORNADO. (Plate I.)



Sketch of the Prostrations found on a section of the Track of the Tornado of June 19, 1835, on the bank of the Raritan, opposite the City of New Brunswick, in the State of New-Jersey.

EXPLANATIONS.—The east bank of the river is here covered with a belt of wood; the latter having a very irregular outline on the east, where it is bounded by a clear field. The line *c c* represents the apparent course of the axis of the tornado: W. west, E. east. Tho large dots on the several figures show the root ends of the trees, which were chiefly a species of cedar. In all these cases of prostration, part of the roots were still fast in the ground. Course of the tornado east. The approximate positions of the several trees are in many cases slightly changed in the sketch, for the purpose of a distinct exhibition of each.

Note.—This bank of the river is intersected by small ravines with wooded margins, one of which is nearly opposite to chain 5, and another is near chain 13, and which cause most of the irregularity in the wooded outline.

ART. IX.—*Notice of a Model of the Western portion of the Schuylkill or Southern Coal-Field of Pennsylvania, in illustration of an Address to the Association of American Geologists, on the most appropriate modes for representing Geological Phenomena;* by RICHARD C. TAYLOR, Member of the Amer. Phil. Soc.; Fellow of the Geol. Soc. of London, and of other Societies in Europe and the United States. Read 9th of April, 1841.

ON the 18th of June, 1830, I had the honor of presenting to the Geological Society of London, and of reading a concise description of two models and sections of part of the mineral basin of South Wales, in the vicinity of Pontypool.

On the present occasion I take the liberty of exhibiting to the Association of American Geologists, at their second annual meeting, a model of the western half of the Schuylkill coal-field, in Pennsylvania. This is, in all probability, the first geological model that has been constructed in the United States; as was, I believe, that of the Welsh mineral district, the earliest of its kind; and as such was received in the exhibition of the Society of Arts.* I have felt anxious, I may say ambitious, to introduce the first American geological model to this Association. It seems necessary to the occasion to make a few explanatory observations, and I desire especially to address some general remarks to this meeting, on the available methods of geological illustration.

Before proceeding to the descriptive details of the present work, and of the region which it represents, I would advert to the extreme applicability of the science of modelling to the purposes of geological elucidation.

During a somewhat active life, embracing thirty six years of occupations connected either with the superficial features of our earth's surface, in various climates, or with investigations of the positions of rock formations, the modes of representing the principal phenomena, and the different systems resorted to for practical illustrations, have, of course, been long and frequently under deliberation. The result, it need scarcely be added, is an increasing conviction of the vast superiority of that method which ad-

* The gold Isis medal being awarded to the exhibitor in 1830.

mits of shewing the objects solidly, in relief; and according to their actual proportions, whenever practicable. I mean the process of modelling areas of country, in preference to any other method of representation; whether by drafts, diagrams, tables, maps, sections or other customary means. Under this impression, and with a view to convey these sentiments in a useful direction, I have made some exertion to complete, for this occasion, a specimen of the art, illustrative of several hundred square miles of interesting country in the interior of this state; but have only during the intervals of the present meeting of geologists, found time to prepare and commit to paper the following observations.

With the best assistance which art can confer, by means of horizontal or vertical or concentric shading, or by the most elaborate arrangement of lines upon a plane surface, to produce the effects of light, shadow, height, depth and perspective, such processes, it is universally conceded, fail to accomplish what is simply effected by modelling. If to these desiderata in geological illustrations, and to those other lines which are indispensable to topographical or local delineation, we add those which are intended to represent the courses and the inclination of strata, and the breadths and separate characters of formations, the difficulties attendant on lucid illustration are heightened, in any process short of modelling.

Whenever the scale, upon which a given area is protracted, is sufficiently large to permit an approximate correspondence between the horizontal and the vertical admeasurements, the effect is perfect. The utility of the work is enhanced, in as much as it combines the exhibition of both transverse and horizontal sections on the field of survey; and illustrates not only the external features and physical geography of the district, but enables the interior structure, the grouping of its mountain masses, the inclinations, bearing, direction, contortions and dislocations of the strata into which those formations and masses are subdivided, to be exhibited in a simple yet very striking and appropriate manner. With the addition of superficial coloring, the pictorial character of the region represented, can be as accurately depicted as in a highly finished landscape. Perhaps even more so; in as much as the positions of all surrounding objects, and of all accessory details, are defined with geognostic accuracy in the one case, rather than imperfectly traced in the other, however experienced may

be the hand and the eye of the artist. The interesting and accurate effect incidental to a picture, thus formed in relief, is apparent enough when the observer brings his eye to the level of any point whatever on the model;—the summit of a mountain, the point of a bluff, or the curve of a river, for instance,—from whence all that he needs in obtaining at ease and convenience a view of the surrounding scenery, is accomplished.

For topographical observations, for rapid reconnoissances, for tracing routes for railroads, for canals, or for ordinary roads and communications, the model system presents facilities for numberless practical purposes, and may be the means of saving a great deal of preliminary labor and expense, on such occasions, in a mountainous or forest district.

In all such regions, it is common to adopt as the best, because they are the most natural and the most permanent, lines of demarcation, the elevated chains, the elongated ridges, the ranges of highlands or platforms which divide the sources of rivers and influence the descent of drainage; or to constitute the rivers themselves, as they flow between these ranges, the boundaries of local and territorial jurisdictions. All of these are particularly and necessarily prominent features in a model; and these, the most sublime and most imperishable monuments in all countries, have with propriety been selected as the most fitting for such conventional purposes. Had a model, however roughly constructed, been in existence to illustrate the physical geography of what is termed "the disputed territory" in the northeast, half a century of embarrassment and conflicting opinions, and local difficulties, might have been saved to the interested parties. It is not too late, even at this hour, to exhibit in this way, all the topographical characters of that region; to represent those great natural features, suggested for lines of international boundary. All the details applicable for this purpose are but now in progress of collection. From their arrangement we may expect to result the clearing up of existing obscurities; a more accurate construction of terms, and the adjustment of important points, now at issue.

Models are peculiarly adapted to the exhibition of geological phenomena. For ordinary convenience of transportation and portability, no doubt maps are best adapted, for the library, or for the use of the traveller. But for public and more enlarged ob-

jects, and for scientific institutions, the more permanent and ponderous form of representation, such as that we have now under consideration, does appear to possess stronger recommendations. We would desire to extend the principle so far as to introduce it in the final elucidation of state geological surveys; convinced that the greatest advantage would result from it. There exists no remarkable or insurmountable difficulty in thus exhibiting, in the distinctest form, the most prominent geological features of the states around us. There appears to be no practical or scientific reason, (pecuniary considerations aside,) why the results of those labors which have proceeded so successfully, and are still prosecuted so satisfactorily, in most of the States in the Union, by gentlemen of high professional eminence, a large proportion of whom are now assembled here,—there appears no reason, I remark, why the vast mass of facts which they have thus so industriously accumulated, should not finally receive this mode of representation. The capitols of Harrisburg, of Richmond, or of Albany, and other seats of local government, might be honorably adorned with instructive geological models of their respective states; and in due time when the great work has so far proceeded in advancement, the Capitol of Washington itself might be enriched by one superb model, in which shall be concentrated those results which so much combined talent has brought to light and reduced to order, and the usefulness of which has been demonstrated.

The illustration of physical geography by means of models has long been practiced in the admirable representations of mountainous regions in Southern Europe, by skillful artists. Many of the European museums contain extremely beautiful models of alpine districts. Some of these even embrace a large portion of Southern Europe; constituting, in fact, maps in relievo, elaborately executed and truly valuable as works of art. For the most part they are designed as pictorial representations of highly interesting regions; without particular reference to their geology, or to the interior structure and arrangement of their elevated masses. Although these much prized models obtained a place in the English collections, as splendid specimens of a peculiar art, the application of that art to economic geology, and to kindred subjects, for which it is especially adapted, has been but little employed in England and its introduction is of comparative recent date. The geological model, for which the Society of Arts conferred their

gold medal, in 1830, was the first which had been exhibited in that excellent institution. A recommendation to adopt the more frequent application of the system, has been occasionally urged by prominent geological authorities. Since the date referred to two most elaborately executed models, on a very large scale, have been exhibited in London; the one represents the field and battle of Waterloo, the other depicts the beautiful lake scenery of the north of England; both of them the result of vast labor and singular perseverance. Those models which, in Germany and some other mineral countries, represent the internal economy of the mines and mining operations, belong to a class extremely useful, but different to that which has given rise to the present memoir.

We come now to the consideration of the model before us. In point of mineral value, of geological peculiarities, of statistical intricacy, and of highly picturesque features, the district here represented in miniature, yet with sufficient faithfulness as regards characteristic distinctness, has perhaps no equal, within a similar area, in America. Its approximation to the tide waters of the Atlantic coast, moreover, confers upon it a commercial value, in connection with the sources of industry and of remuneration for labor, manifestly within its limits. We are justified in adverting to these circumstances, because the useful results, and the beneficial application of science in economic geology, form legitimate objects of associations like that which I have the honor of addressing.

The area here illustrated comprehends seven hundred and twenty square miles; being in length forty five miles, and in width sixteen miles. It extends in breadth from four miles above Harrisburg, northward, to Millersburg on the Susquehanna, at the junction of the Wiconisco railroad. In length it reaches from the western extremity of the Cove mountain, on the west side of the Susquehanna to within eight miles of Pottsville. It comprises the two forks into which the Schuylkill coal-field separates, opposite to Pinegrove, in the Swatara region. The northern fork or branch extends to the Wiconisco Coal Company's mines at Bear Gap, and the southern branch stretches towards the Susquehanna in a southwest direction, to within about a mile of that river. The coal formation along several miles of the western portion of this lower fork, is reduced to a narrow ridge, which can scarcely be expected to contain coal to any valuable

extent. Both the branches alluded to are bounded or enclosed by corresponding mountain ridges; the strata of which, composed of the inferior red shales and of a numerous series of sandstones and conglomerates, underlie the coal measures and the upper red shales. The coal strata in these separate branches or basins dip, for the most part, to their respective centres.

The horizontal area is protracted as a scale of two inches to a mile; the data for which have been derived from a variety of public and local surveys. In the vertical scale we have been enabled to approach so near to the horizontal as two of the former to one of the latter; an approximation which is more close than is usual in such works. And here it may be permitted to apply some remarks on the construction of diagrams.

It has been customary with most geologists, and I believe almost universally with civil engineers, where the bases of their sections are considerably extended, to adopt a much larger scale for the perpendicular than for the longitudinal dimensions. Consequently, the diagrams, so drawn, amount to absolute distortions, and manifestly convey very inaccurate ideas. The proportions of relative heights and lengths are thus so grossly caricatured, that they bear but distant resemblances to what is intended to be represented. The inclinations of strata are changed from moderate angles almost up to vertical; the altitudes of hills are stretched to the eminence of lofty peaks; rounded secondary mountains assume the form of attenuated spires; gentle undulations become craggy steeps, and the ordinary surface of a country is thus metamorphosed into a region harshly broken into pinnacled spires and alpine crests, and steep and fathomless gulfs—a hideous burlesque upon the actual aspect of the district represented, or rather misrepresented.

In constructing geological diagrams I have, for some time, ceased to make any difference between the horizontal and vertical scales. At any rate I have endeavored, as closely as may be, to adhere to that principle. If the drawings be executed with delicacy they rarely require a deviation from the rule; and I would respectfully recommend an adherence to it, among my geological friends, particularly in relation to the state surveys, where comparisons of sections are continually needed. We shall then, and not till then, possess something like uniformity in the representations of similar things. So long as the distorting principle

is tolerated we shall continue to convey and to view every thing under a false medium, and shall describe objects under every shape but their real one. Geological sections, if drawn with suitable care and with the nicety that such works demand, particularly if they be engraved rather than lithographed, may be made perfectly distinct at a very small vertical scale. Detailed sections of particular portions, on a larger scale, can readily accompany and elucidate the general section. The present writer has constructed sections of many hundred miles in this country upon a scale, both vertical and horizontal, or very nearly corresponding, as small as five miles to an inch, and yet has exhibited all important features therein. The system is clearly the right one, and ought to be followed. It is the only one, in fact, which can be made to exhibit the true inclination of the strata, the real bearing, position and magnitude of the formations and their relation to each other, and furnishes the means of measuring the thickness of those masses. In modelling also, although not always attainable, it would be equally desirable to approach as nearly as possible to the same rule.

I have dwelt the more strenuously upon these methods of illustrating geological phenomena, with a view to attract the attention of gentlemen who are about to place before the public, for the benefit and instruction of us all, the result of their respective labors in the field. I would take the liberty of earnestly soliciting their attention to a matter which we all admit is extremely desirable,—namely, uniformity in the process and modes of representation. Wherever it is possible, let similar scales be employed for the geological sections of different states. Wherever it is practicable, and to a very great degree it already is, let similar colors represent similar formations, wherever they occur. If at this stage we cannot yet settle that extremely difficult point, that "consummation devoutly to be wished," a common nomenclature, let us approach it as near as we can, by the use of common symbols, as a temporary substitute for a common language.

I think these are matters on which the present meeting might, with perfect propriety, enter. Let it be borne in mind that the accumulation of facts is one thing—a desideratum of primary importance, certainly. But the science of putting together those materials—the exemplification of those facts—to effect the purposes of geological elucidation, to reach the understanding, to

impress on the mind and memory, is no mean part remaining to be performed. Let it be remembered that facts are comparatively useless without arrangement; that they are valueless if they are not presented to the senses in an intelligible and accurate form. The elements wherefrom to erect a geometric figure may be before us, but until we have truly constructed that figure from those elements, our impressions as to its form and proportion are necessarily vague and feeble. The materials wherewith to construct a house or a ship may be all prepared with strict regard to their individual dimensions, but as separate members they convey to us no idea of the actual form of that house or that ship. It is the art of the builder then to put together those materials; and, in like manner, the geologist, or the geological draftsman, or the modeller, has to exercise his art, in putting together and exhibiting in correct forms, the details he has labored so hard to collect. Above all things, let him avoid distortions in drawing. It is incumbent upon those who undertake to enlighten and instruct others by diagrams, to exhibit those diagrams in true, and not in false proportions. The master can no more hope to convey to his pupil a right idea of a cube or a square, for instance, by representing in his diagram its height four or five times its breadth, than can the draftsman in our science, expect to convey correct notions of geological arrangement by a similarly defective process.

Returning to the model before us. The local *elevations* above the level of tide, have been ascertained at a sufficient number of points, particularly in the coal districts, to convey the prevailing characters of the country. A number of these heights, are marked on their proper sites upon the model. All of these were found by spirit level and positive admeasurements. The Pennsylvania canal on the east bank of the Susquehanna river, where it cuts through the second mountain, is three hundred and twenty seven feet above tide level in Chesapeake Bay. The Swatara river, above Pinegrove, passes through the same mountain, thirty one miles to the eastward, at the height of six hundred and nine feet above tide. The prevailing elevation of the ridges which form the north and south edges of the southern coal basin, is sixteen hundred or sixteen hundred and fifty feet above tide water. As a general remark, when casting a glance over the area here represented, we cannot but be struck with the comparative uniformity in their elevations, and the extensive maintenance of

those levels along the crests of the ridges, when not broken by transverse fissures. The Blue or Kittatinny Mountain, the southernmost of these nearly parallel ranges, is probably the highest. The coal range is next in elevation, and there is some lofty ground, forming Short Mountain, between Peter's and Berry's mountains.

Geological Features.—Under this head we shall here be very brief; because that subject is not the primary object of this address; and because the region has received or will receive, ample investigation by the state geological survey, with all the combined advantages resulting from the official resources, the science and the experience of its able conductor. The results of that great work it would be premature to anticipate. The positions of the various formations and of the respective members of the groups of strata, within these limits, have already been indicated in the annual reports of Professor Rogers.

In contemplating this region, it appears to us that its most interesting features are attributable to the undulating and broken or upheaved character of the formations, by which process some of them are repeatedly brought to the surface, in long elevated ridges, and again dip at high angles and form basins which enclose or support the superior strata—the carboniferous series being of course the highest. These circumstances confer a remarkably picturesque character upon the scenery, particularly where these parallel ridges are intersected by the Susquehanna, the Juniata, and the Swatara rivers. No part of Pennsylvania is so rich in pictorial beauty as the borders of the noble Susquehanna, or has furnished so many subjects for the skill of the painter.

The spectacle here presented, by this river, cutting across in its singular passage, nearly at right angles, through so many ridges of extremely hard rocks, would of itself furnish a theme for geological speculation. Phenomena like these are well illustrated by the mode of representation we have adopted. The numerous cross fractures marked by the frequent gaps through the mountains and by the remarkable ramifications of the Swatara, in the Pinegrove coal region, could by no other process of exhibition be rendered so intelligible.

The Coal Formation.—It forms no part of the plan of the writer to encumber this communication with minute details. With regard to the southern branch, more especially, it is the less neces-

sary, as they have been recently published, at considerable length, in the form of reports to the proprietors of the soil.* What remains to be added here, under this head, will occupy a brief space.†

Ranging along the southern margin of this coal-field, appear nine principal transverse sections, "gaps" as they are locally termed, which cut through Sharp mountain. Through these ravines, many hundred feet in depth, the drainage of the coal area descends southward; and by the same avenues the highly inclined coal seams are intersected. The height at which these coal seams can now be reached within the gaps, without expensive tunnelling, varies from eight hundred to eleven hundred feet above the level of the sea; and as the summit attains an elevation of sixteen hundred and fifty feet, there are therefore from four hundred to eight hundred and fifty feet, measuring perpendicularly, of coal, capable of being worked, above those points of intersection.

The number, thickness, compactness and density of these coal seams increase as we pass eastward along the counties of Dauphin, Lebanon and Schuylkill. At the same time, and in a corresponding degree, or rather in a reverse ratio, the amount of bituminous and volatile matter, contained within the coal, diminishes; passing from a bituminous or semi-bituminous coal, which yields a bright blazing fire, and at some points is convertible into coke of good quality, to a compact anthracite, on the borders of Schuylkill county. This fact is exemplified in the series of analyses made on behalf of the proprietors and published in 1840, and subsequently by another series, recently embodied in the state geological report.‡ The prevailing breadth of this southern fork, measuring from red shale to red shale, is about a mile; except towards the western termination, where it is only about one thousand feet to one thousand and two hundred feet wide, for three or four miles. In this range the lower conglomerate, interposed in great thickness towards the eastern extremity of the Schuyl-

* Vide report on the coal lands, mines, &c., of the Dauphin and Susquehanna Coal Company, by Richard C. Taylor, president of the board of directors. Report of the geological examinations, &c. of the Stony Creek Estate, in Dauphin and Lebanon counties, by Richard C. Taylor. Philadelphia, 1840.

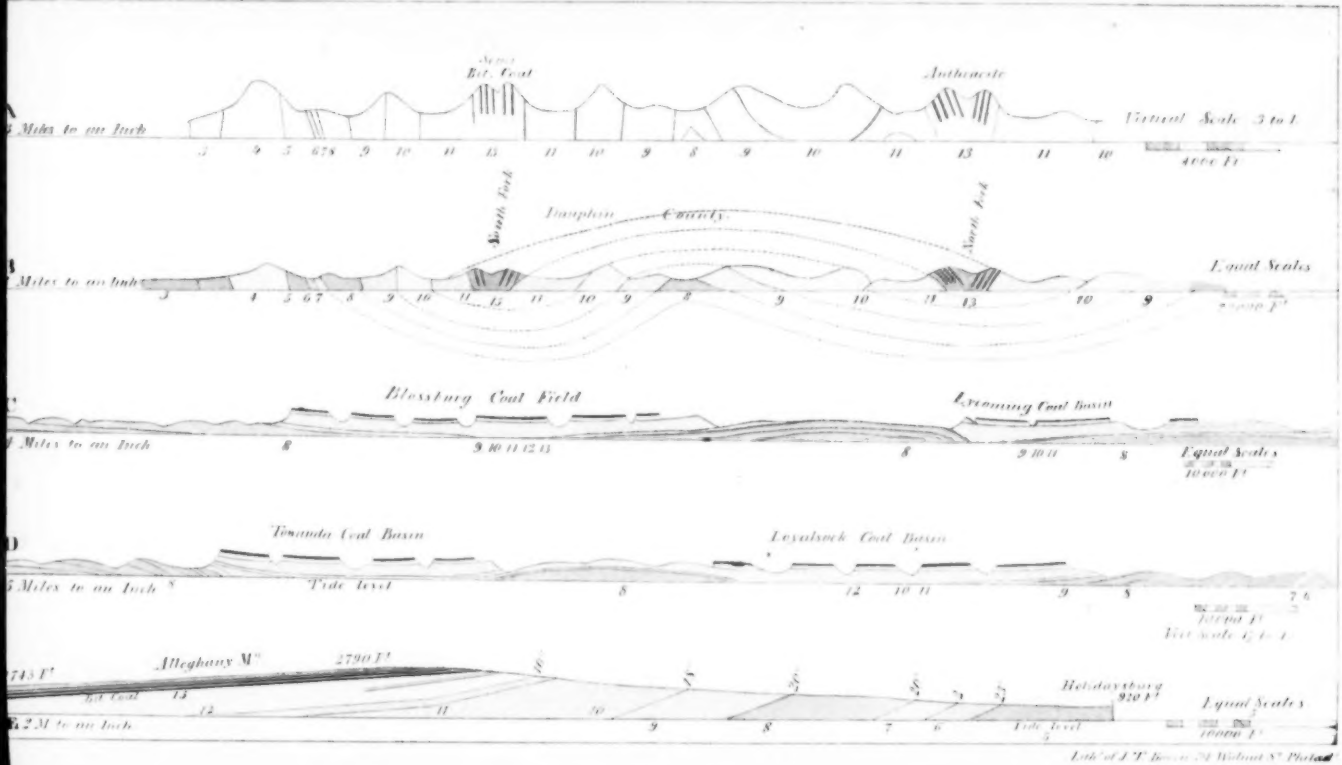
† At the request of the author, we have, in printing the memoir, omitted many details which were embodied in the original; because without the assistance of maps and diagrams, they could not be rendered sufficiently intelligible to the reader.

‡ See also a series of analyses of coals from this region, published in Vol. XL, p. 373, of this Journal, by Mr. M. C. Lea.

kill coal basin, between the coal beds and the red shale, has thinned out and at some points appears to be altogether absent. The greatest amount of coal which has been proved at this branch, is at Blackspring gap, where eight southern and seven northern seams have been explored, the aggregate thickness of which is ninety feet.

Passing over to the *northern branch* of the main coal-field, a fine series of beds occurs, some of them being of considerable thickness. The anthracite here is of excellent quality. From the Swatara region eastward for several miles, there has been but little exploration of the numerous coal seams known to exist there; there being no convenient mode of communication, by canal or railroad, completed in that quarter. Argillaceous carbonate of iron, in beds and detached masses, prevails in this coal region; to what extent, however, we cannot say, as their investigation has hitherto been but a secondary object.

The foregoing notes are, it is conceived, sufficiently explanatory of the prevailing characters of the district. Did it possess no other peculiarity or attraction than that derived from the remarkable arrangement, or rather derangement, of the formations which it comprises, it might still claim your attention, as an area of high geological interest. Viewing it with reference to its growing importance as a mineral country, favorably circumstanced, we have little cause for apprehension that the labors of the artist have been employed on a barren and profitless field. Already have nine or ten chartered companies for coal and mining operations located themselves here. Already several furnaces and forges have been established in its vicinity. The Pennsylvanian, the Wiconisco and the Union canals, traverse within its limits. Two or three railroads are already in full communication with its collieries, and charters for five or six others have been procured from the legislature. As relates to the region we have been considering, we are but on the threshold of improvement. The industry of man has but recently been put in requisition within its borders. But experience has already informed him that the once despised, chaotic, impassable wilderness, teems with treasures more precious to him, perhaps, than gold. The labors of the geologist, be they local or general; be they for private or for public objects; for individuals, for associations, or for the community at large,—cannot fail to develop new and beneficial results wheresoever directed, in such a field. To have had some share in the attaining and the



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distribution of this knowledge, and to have contributed any aid to the great cause of economic geology, is a gratification which is worth no small exertion to acquire. It has proved, let me add, in all sincerity, the strongest inducement to perseverance in the work now before the association.

At the request of some members of the association, I have annexed to the foregoing memoir a few illustrative sections constructed on a variety of small scales, with the intention of exhibiting the practicability of using even minute vertical scales, in geological demonstrations. They are as follows:

Fig. A, is a section whose horizontal scale is three miles to an inch, and the vertical scale 5280 feet to the inch, being in fact in the proportion of 3 to 1. This, although less distorted than is occasionally the case, it being easy to point out examples where the proportions are 6, 8, and 10 to 1, is drawn to show the contrast to the section B beneath it, where the proportions are equal, the horizontal line and the area illustrated being similar.

Fig. B. Section at three miles to an inch, both vertical and horizontal, of the same ground as Section A, and in fact a transverse section of the model, which has been described in the foregoing paper.

Fig. C. Section protracted at four miles to an inch, both vertical and horizontal It shews the position of two of the detached Pennsylvania bituminous coal basins.

Fig. D. Section at five miles to an inch. Here there is a trifling increase, amounting to one half only, in the vertical scale, viz. 1½ to 1. It also exhibits two detached coal basins in Pennsylvania.

Fig. E. Section at two miles to an inch, on equal scales. This projection is sufficiently large to admit of characteristic details. It is a profile of the Alleghany mountain, descending eastward; also in this state.

As I have not conveniently at hand, examples of sections having the altitudes above tide level, drawn by other authorities, it was necessary to resort to the materials which happen to be in my possession, and for the most part prepared from personal observation. I hope they are sufficiently accurate for the purpose designed.

To render these experimental drawings more useful for comparison I have inserted Professor H. D. Rogers's numbers of the respective formations. With regard to the colors adopted, they are not proposed as standards, but are simply those which I have been accustomed to employ; differing very little, I perceive, from those used by the gentleman last mentioned.

Before closing this subject, permit me to allude to the new geological map of England and Wales, by Mr. Greenough. As a finished specimen of art, it is probably the most beautiful production of the age, and may with great advantage, be consulted, for the extreme clearness of its details.

ART. X.—*A Notice of "Indian Cyprinidæ," being the second part of the nineteenth volume of Asiatic Researches, by Mr. John M'Clelland.* Read Feb. 17, 1841, to the Boston Society of Natural History, by D. HUMPHREYS STORER.

WE are almost entirely ignorant of the fresh-water fishes of many of those countries, whose marine species are tolerably well known to ichthyologists. The reason is obvious—the smallest sea-port has its market constantly supplied with those species which are used for food—the great proportion of such species, are taken along the shores of the different countries, and but very few fluviatile fishes are considered of sufficient value to be procured—so that, we are compelled to learn from the zealous naturalist, who is actuated by other motives than the expectation of pecuniary reward, the characters and habits of such species as inhabit the streams and rivers and lakes. He therefore who attempts to elucidate a subject so desirable to be known, recommends himself by the mere effort, to our regard—should he succeed in his attempt, we ought not to withhold our gratification.

Agassiz, whose name is a guarantee of the value of his labors, is at this moment preparing a magnificent work upon the "Poissons d'Eau Douce de l'Europe Centrale." In our own country, Dr. Kirtland, like a true naturalist, with an enthusiasm proportionate to the obstacles he encounters, is endeavoring to present you with the "Fishes of the Western waters"—and I feel confident, when his entire paper shall be published, although future research may, and undoubtedly will point out errors, that it must be invaluable to the American ichthyologist.

While these observers are prosecuting their enquiries, we are surprised to receive an elaborate paper upon the "Indian Cyprinidæ," read to the Asiatic Society in Sept., 1838, and published in their "Researches" the following year. This paper, to which I would now call your attention, was prepared by Mr. George M'Clelland, assistant surgeon in the Bengal medical service. Our author was induced to undertake the elucidation of this subject, by perceiving that Cuvier had adopted only such of the Indian Cyprinidæ as were figured in Dr. Buchanan's work on Gangetic Fishes—leaving the remaining three fourths of the species described in that work, as not well determined—and feeling satisfied that these

descriptions of Buchanan were so general that they could not by any one be distinguished, he resolved to make the attempt to identify them, by collecting all these species and minutely studying their characters. "After perseverance for the better part of three years," to use the words of our author, "occasionally giving it up in despair, I succeeded in identifying most of the species unfigured by Buchanan, as well as in having made two series of finished drawings of them, one set for England and one for India." After his paper was ready for publication, our author learned that some of Buchanan's drawings of his Gangetic Fishes, were in the government house at the botanic garden in Calcutta—and upon investigation, found a collection "amounting to one hundred and fifty beautifully executed, and including nearly all the unpublished species on which my painters had been so long employed, with the specific names in Buchanan's hand-writing marked under the figures, so as to leave no doubt or difficulty in referring them to corresponding descriptions in the Gangetic Fishes." Fortunate indeed was it for science, although gross injustice to Buchanan, that these drawings should have been thus long concealed; had all the figures appeared in his "Gangetic Fishes," they would have supplied the deficiency in his descriptions, and the rich volume before us, would have never been undertaken. Now, after having for years examined the swamps and stagnant pools, and the mountain streams of India—after having enlisted his numerous friends in his service, and possessed through their efforts and his own, not merely all the species described by Buchanan, but many previously unknown—Dr. M'Clelland is not satisfied merely to cry out *ευρηκα*, but embodies here a great amount of information obtained during his researches, and throws new light upon the ichthyology of the east.

The Cyprinidæ, are arranged by Cuvier in the "Regne Animal," as the first family of the Malacopterygii abdominales—and are characterized thus—they are "recognized by the slightly cleft mouth; the weak jaws, generally edentated, and whose border is formed by the intermaxillaries; by the deeply dentated pharyngeals which compose the trifling armature of the jaws, and by the small number of the branchial rays. Their body is scaly, and they have no adipose dorsal, such as we shall find in the Siluri and in the Salmons. Their stomach has no cul-de-sac, neither are there any cæcal appendages to their pylorus. Of all the fishes, they

are the least carnivorous." This family is divided by Cuvier into seventeen genera, characterized for the most part by the form of the mouth, and the position of the dorsal fin. After a minute examination of the digestive apparatus of these fishes, Dr. M'Clelland has pointed out a natural arrangement, which goes far to simplify their study. He ascertained that upon the greater or less development of the intestinal canal, he could determine the food taken by the different genera—whether it was animal or vegetable; and that the position of the mouth corresponded with this arrangement of the canal. That such of the family as lived entirely upon vegetable food, possessed the greatest development of the intestines—and their mouths were horizontal or directed downwards; and that those which lived upon insects, had the least development of these organs—and their jaws were directed upwards.

He has accordingly formed three subfamilies, into which he divides the Cyprinidæ. The first, he calls *Pæonominæ*, or herbivorus Cyprins—from *poionomos*, that feeds on herbs. This subfamily is thus characterized—"mouth slightly cleft, either horizontal or directed more or less downward. The stomach is a lengthened tube continuous with a long intestinal canal; colorus plain; three rays in the branchial membrane. *Obs.* Their food consists chiefly of confervoid plants and other productions of the vegetable kingdom." The *Pæonominæ* contain five long established genera; viz. *Cirrhinus*, *Barbus*, *Cyprinus proprius*, *Gobio*, and *Gonorhynchus*. From the Barbels, our author has formed a subgenus, which he calls *Oreinus*, from *Oreinos*, pertaining to mountains.

The second subfamily is called *Sarcoborinæ*, from Σαρκοβορος, carnivorous. This subfamily is composed of five genera. Two of these genera were previously established, viz. *Leuciscus*, (Klein,) and *Abramis*, (Cuv.)—three other genera are formed by our author. The first of these, he calls *Systomus*, from *Systomos*, that has a narrow mouth. *Characters.* "Intermaxillaries protractile, dorsal and anal short, the former opposite to the ventrals and preceded by a spinous ray; body elevated, and marked by two or more distinct dark spots, or diffuse spots either on the fins or opercula, prominence on the apex of the lower jaw obscure."

The genus *Perilampus*, from *Περιλαμπω*, to irradiate, or shine brilliantly, is thus distinguished. "Head small, obliquely raised above the axis of the body; dorsal placed opposite to a larger anal; apices of the jaws raised to a line with the dorsum, which is straight; the ventral margin is much arched; sides usually streaked with blue; fins without spinous rays. *Obs.* In this genus the intestine is small, and very little longer than the body. The species all subsist exclusively on insects, which they seize by leaping above the surface. They vary from two to four inches in length."

The third genus of our author, is *Opsarius*, from *οψαριον*, *pisciculus*, a small fish; its characters are thus defined. "Mouth widely cleft; body slender, and usually marked with transverse green streaks or spots; dorsal small without spines, and placed behind the middle, and long, lower margin of the body more arched than the upper. *Obs.* Intestine very short, and extends almost straight from the stomach to the vent."

In the genera of this subfamily, the mouth is situated directly opposite to the position it had in the former. They are all insectivorous—the *Opsarions*, which also devour smaller species of fishes, particularly *gudgeons*, are so voracious, that "it is no uncommon thing to find an Opsarion so overgorged that the tail of its prey remains protruding from the mouth, to be swallowed after that portion which is capable of being received into the capacious stomach is sufficiently digested to admit of the introduction of the remainder." Another striking distinction between the individuals of these two subfamilies, is pointed out to us in their colors. "The whole of the subfamily *Pæonominæ* are remarkable for their uniformly plain colors, consisting of olive green, bluish gray, or brown, extended along the back, and softened off on the sides so as to leave the lower surface of the body an impure white, partaking more or less of the colors of the back." "Of the species, not one possesses a brilliant spot of any pure color." But as we leave the herbivorous group and enter the carnivorous, we find numerous bright, dark spots, and the operculæ and fins to be stained with yellow and red, in deep and natural tints; and the more carnivorous the genera the more remarkable is the brilliancy of their colors.

The third subfamily, is called *Apalopterinæ*, from *απαλος*, soft, and *πτερον*, a fin or wing. It "consists of those genera, the spe-

cies of which have either elongated cylindric bodies or flat heads, as the *Loaches* and *Pœciliæ*. They are without spinous rays in any of the fins, the intestine is short, and enveloped in a copious mucous secretion; three to six rays in the branchial membrane." There are four genera.

The first genus of this subfamily is called *Platycara*, from *platus*, broad, and *kara*, head. Its characters are "head flat, with the eyes placed on the upper surface, fins thick and opaque, pectorals large, anal small, caudal bifid, mouth without teeth and placed on the lower surface of the head, three rays in the branchial membrane. *Obs.* The stomach and intestine form a continuous fleshy tube, not much exceeding the length of the body; they are found in elevated mountain streams."

The second genus is *Psilorhynchus*, from *psilo*, thin or attenuated, and *rynchus*, a snout or beak. It is distinguished by "muzzle elongated and flattened, eyes placed on the edges of the head, mouth small and suctorial without cirri, opercula small, caudal bifid, dorsal opposite to the ventrals."

The third genus *Pœcilia*, includes the genera *Pœcilia*, *Lebias*, *Fundulus*, *Molinesia*, and *Cyprinodon*, contained in the "Regne Animal." A single subgenus is formed from this genus, which is called *Aplocheilus*, from *Απλος*, simple or single, and *χειλος*, the lip, and having the following characters—"intermaxillaries fixed, apices of the jaws broad, flat, and directed upwards; five rays in the branchial membrane; fins transparent. *Obs.* A short dorsal is placed opposite to the last ray of a long anal, the ventrals are very small; the intestine and stomach form together a small tube scarcely longer than the body."

The fourth genus *Cobitis*, (Linn.) is divided into two subgenera; the first, our author calls *Cobitis propria*—here, we find the "caudal entire, large, and ornamented as well as the dorsal, with bars or spots; prevailing color of the body, various shades of brown, disposed in more or less dense nebulæ." To the second subgenus, our author has given the name of *Schistura*, from *Σχιστος*, split or separated, and *ουρα*, cauda. Its characters are "caudal bilobate, dorsal and ventrals opposite, and short; with or without suborbitar spines; sides ornamented with fasciated bars, mostly green. *Obs.* The intestine is somewhat longer than that of the true Loaches, (*Cobitis propria*,) being usually reflected once upon the stomach."

We have thus exhibited a mere outline of the important improvements proposed by Dr. M'Clelland in the natural arrangement of the *Cyprinidæ;* but besides his classification, our author has presented us a general synopsis of the species, and illustrated them with lithographic figures, from drawings mostly his own, all of which are sufficiently well executed to convey accurate ideas, and many of them are highly creditable. Besides furnishing nineteen colored plates, containing nearly one hundred figures, the volume before us is also enriched by a single plate, exhibiting the peculiar forms of the jaws in several genera of the subfamily *Pæonominæ*, and the differences of form and proportion of the alimentary canal in each of these great subfamilies.

Almost one half of the entire volume is devoted to an "account of the species," and it constitutes by far the most interesting portion. We here find, not merely a scientific description of each fish, with its Latin and native name and its geographical locality, but its habits are elucidated with great clearness and evident acquaintance; its value as an article of food, from its delicacy or abundance, and the possibility and importance in many instances of its being transported from one locality to another for economical purposes, are pointed out with the zeal of a philanthropist; while the perseverance and fidelity with which our author has studied the minute anatomy of his subjects, must claim from all readers their admiration.

Published as this treatise is, by a society whose "Transactions" have become so voluminous, and are with difficulty procured, it cannot be extensively circulated or known among American naturalists; I would notice therefore a few points of general interest, selected from the portion of this paper just referred to.

In some parts of India, many of the species of fishes are found in immense quantities. Our author, after describing *Cyprinus chola*, remarks: "Casting a net into a pond in Middle Assam, not presenting any remarkable appearance of containing fish, about one hundred and twenty were brought up at a single draught"—(nine species captured are here mentioned). "The extent of the pond may have been four hundred yards, and that of the net three yards; and supposing half the fish to have escaped from under the net, the number in the pond would have been thirty two thousand. When we consider the vast extent of surface occupied by waters equally productive, both in Bengal and As-

sam, we may form a conception of the inexhaustible supply of fish that might be procured for consumption in other parts of the country where they are less plentiful." p. 384. Several species are useful not merely as an article of food; speaking of *Cyprinus cursis*, Dr. M'Clelland observes, "It is a beautiful fish, common in Bengal and Assam as high as Sudyah, but being full of bones, is little valued as an article of food. If it be less useful in this respect than other Cirrhins, it is more serviceable than we are aware of, in common with the numerous Gudgeons, in clearing the indolent waters of the plains from a redundancy of vegetation with which they would otherwise be choked up." p. 330. Many of the species are of great value as excellent and nutritious food, and might be rendered infinitely more so, could they be salted when taken, or be transported and allowed to propagate in other portions of the country. Thus the *Barbus megalepis*, which is an admirable fish, is "peculiar to remote, unpopulated districts, where no attempts are made to cure; they are consequently lost to the wants of other places, where an unlimited demand for dried fish must prevail at all seasons, but particularly during the rains and hot weather, when fresh fish become scarce. There can be no doubt that if some relaxation of salt duties could be made in favor of those who would embark in such a business, a profitable and useful trade might be established to a far greater extent than we can at present form any notion of. The season for fishing is short, and without the means of saving more than can be consumed when fresh, the fishermen have nothing to stimulate them to any exertion beyond that of earning during their brief season, a sufficient sum to support them during the rest of the year. Had the fisherman the means of preserving the results of his labor, his chief market would commence when the fishing season ends, and his industry would then become a permanent benefit to himself and to the country at large. Sea fisheries would be of still higher importance, although neither should be neglected. The cold season, from November to February, is the time at which fishes are chiefly taken; the waters being then low, the fish are confined to narrow channels, and are often completely cut off from the larger streams and left in pools, in which they are easily secured. When passing Solano Mookh with the Assam deputation in January, I saw boats laden with most of the five kinds of Barbels just described, from

one to two and a half feet in length; but as Suddyah, the nearest market at which they could probably be disposed of, was thirty miles distant, and a strong current to be opposed in reaching that place, and no means of curing the fish, the owners entertained little hopes of realizing any thing whatever by them." p. 339.

To the important subject of transporting fishes from one pond or river to another, our author has given much attention, and his suggestions are worthy the notice of the government of India. We are told, that "throughout the Mysore country, as well as in many of the western provinces, large tanks or reservoirs occur, many of them from three to thirty miles in circumference, and being indispensable for irrigation, may be supposed to be nearly universal in all populous districts not watered by rivers. These reservoirs are considered by the honorable Col. Morison, C. B. as among the greatest national monuments to be found in India. They are capable, according to Buchanan, of supplying water for from eighteen months to two years, and thus of maintaining the surrounding crops should no rain fall within that period.

"They are drained by an ingenious system of sluices and aqueducts of the most simple but complete construction, which afford a perfect control over the distribution of the water. During the dry season they are all pretty much exhausted, and may, if necessary for repairs, be left perfectly dry. This would afford an excellent opportunity for destroying crocodiles and all the various destructive fishes, sparing only the more profitable kinds, which are limited to two or three species only; and by repeating this operation for several seasons, or as often as may be necessary, all but those we wish to propagate would soon be exterminated.

"By a wise law of nature, the carnivorous animals of every class are less prolific than the harmless, and may therefore be the more easily subdued. Nearly all the destructive fishes are viviparous, bringing forth comparatively few young; whereas, the more profitable kinds, or those which should be the objects of our care, are all oviparous, and bring forth their young from spawn.

"A single female carp weighing only nine pounds, has been found by Dr. Locke to contain no less than six hundred thousand ova; and by Schneider, one, ten pounds weight, was found to contain seven hundred thousand ova or eggs.

"The fecundity of the *Ruee*, *Catla*, and *Mrigala*, has not yet been ascertained, but from their close affinity to the carp we may

suppose them to correspond in this respect with that species; the question, however, is one that may be easily ascertained by weighing a grain of the roe and ascertaining the number of globules it contains, while these will be to the whole roe what one grain is to its entire weight. The result will show that these species are capable of yielding, by their extraordinary fertility, a source of food as inexhaustible as the sands of the ocean, could we only bring their propagation and the safety of the young sufficiently within our control.

"In the reservoirs above described, we have every facility for effecting this object on a scale of great magnitude, without in any way interfering with the other uses of the water." p. 458, et seq. "The only alteration in the present form of the reservoirs to adapt them to the purposes in view, would be to enclose the lowest portions of the bottom of each with stakes long enough to reach above the highest surface of the water, and close enough together to prevent the entrance of crocodiles, otters, and the like, should any such exist in the neighborhood. The spawning season of the *Ruee* and other *Cirrhins*, appears to be in the dry weather; the contrivance here suggested would therefore protect them at that time, and if there should be any danger of the whole of the water drying up, wells of sufficient size and depth might be formed within the enclosure, to which the fishes would retire during droughts, while the shallow waters around the wells would afford space enough for the deposit of spawn.

"Much of our success would depend on keeping those enclosures as free as possible from all but the species we desire to propagate. At the commencement of the dry season, before the fish begin to enter the enclosure, the interval between the stakes might be closed with straw, and as the water becomes sufficiently low without, most of the rapacious kinds may be removed or destroyed; none should be allowed to remain but that species alone which may be the object of our care. This done, the only further attention necessary, would be to save the fish in the enclosure from birds during the remainder of the dry season.

"Should our success be complete, from every moderately sized female *Ruee* we should have, on the commencement of the rains, from five to ten hundred thousand fry, which, as the waters rise, would be quite able to take care of themselves till the next season, when it would be necessary again to destroy the rapacious

kinds as before." "On the fishes of Bengal, Assam, and other provinces subject to the inundations of the larger rivers, we can exercise no control, nor is it desirable that we should, even were it in our power, the supply of fish being plentiful and constant enough; but in the higher parts of the plains, near the foot of the mountains, where the larger *Cirrhins* and *Barbels* retire during the dry season for the purpose of spawning, fisheries might be carried on with advantage to a considerable extent." p. 461.

As some species of the Indian fishes are found only in clear and rapid streams, they would not probably thrive well, if at all, in tanks where the water would be still. Should it be considered desirable to propagate to any extent such species, our author suggests, that "the most suitable *vivarium* for such species might be formed by stopping up a clear mountain stream to a certain depth, and filling the irregularities of the bottom with sand, gravel and stones; there should be a current in the water, and to prevent the escape of the fish, a grating should be fixed below; at the opposite end a stronger grating, if necessary, to prevent the introduction of rubbish during floods, as well as the escape of the fish, should the fall not be sufficient for the latter purpose." p. 347. Occasional remarks are found relating to the altitude at which fishes are known to live in India. Speaking of the *Oreinus guttatus*, Dr. M'Clelland observes, it is found "in rivers in different parts of Boutan, between the elevation of two and five thousand feet. It may occur higher, but Mr. Griffith remarks, that in valleys above five thousand feet, though fine, clear streams are common, yet fishes of any kind do not occur in them, and the natives assured the mission to which he was attached, that no fish existed at such elevations." p. 345. The *Gonorhynchus petrophilus* "inhabits streams in Kemaon at an elevation of six thousand feet above the sea, and has been observed by Lieut. Hutton at similar elevations in the mountains north of Simla, as well as by Dr. Campbell in Nipal." p. 371.

And again, "whether any other kind of fishes may yet be found in still higher altitudes than those at which the Gonorhynchs and Mountain Barbels disappear, is a problem in the distribution of this class of animals, that travellers in the Himalaya and other lofty regions must decide. In the limpid streams which Mr. Griffith passed with Capt. Pemberton, at elevations of from six to eight thousand feet in Boutan, no inhabitants were found;

and both here and at Simla, as well as in Kemaon, the Gonorhynchs and Mountain Barbels have not been found at greater altitudes than six thousand feet above the level of the sea, where we may presume they disappear; but from that altitude downwards to the plain, they constitute the prevailing forms that have hitherto been met with in the waters." p. 369.

It is well known that several of the marine fishes, under peculiar circumstances, produce derangement in the systems of those who eat them; it appears that similar affections are the result of feeding upon some of the Indian Cyprinidæ. Thus we are told "that many of the natives abstain from the use of the *Cyprinus cursis*, imagining that if eaten on the same day with milk it will occasion a disease called elephantiasis." p. 329.

The *Oreinus progastus* "is said by the natives of Assam to occasion swimming of the head and temporary loss of reason for several days, without any particular derangement of the stomach. It is the most herbivorous of the Barbels, and like some of the Gudgeons, tends rapidly to decay after death, and in the abdominal cavity a copious oily secretion is found, which is probably the cause of its bad effects." p. 344.

Our author observes, that in some species "the whole of the abdominal viscera float in a dark, oily kind of fluid;" and he remarks, "that either this fluid, or the great proportion of vegetable matter contained in the intestines of the gudgeon and Gonorhynchs, tends rapidly to putrefaction; to which cause, as well as to the neglect of removing the viscera from those species immediately after they are caught, I ascribe the bad effects which have by some been observed to result on certain occasions from their use." "Mr. Bruce, of Assam, also mentioned to me, that he knew of instances of indisposition supposed to be occasioned by a variety of Bangon. All Bangons and Gonorhynchs should therefore have the viscera removed soon after they are taken, and the dark, oily fluid washed away; when, if it be necessary, they will keep fresh as long as any other kind of fish; but if this be neglected, the stomach rapidly putrefies, in which state, if it be necessary to use these fish, the thin parts adjoining the ventral fins should be removed." p. 371.

I might proceed to point out much, which could not fail to interest you, but it was not my intention to present an elaborate paper. I wished merely by glancing generally at the work before

me, to show you how creditably the author has performed his task; he deserves, and will I trust, receive in the pages of the scientific journals of his native country, a faithful critique.

Before closing this report, I would revert to a single circumstance which is highly honorable to Dr. M'Clelland. It appears that Dr. Buchanan, during a long residence in India, had made a large collection of papers and drawings illustrating the natural history of that country, which were taken from him, as he was about to leave India, by the Marquis of Hastings, and deposited in the government house at the botanic garden in Calcutta—so that he was obliged when he reached Edinburgh, to publish his Gangetic Fishes without most of his plates. Dr. M'Clelland, when he finally knew of the existence of these drawings, was surprised to ascertain that Hardwicke in his "Illustrations" had freely copied many of them, without giving the slightest credit to Buchanan, and had even annexed to them new names, although the names of their discoverer were attached to the drawing, and in his own handwriting. But the names were not merely changed; "during the twenty years Buchanan's drawings lay at the botanic garden, before they were transferred to Hardwicke's Illustrations, many of the colors appear to have undergone a change, such as light blue and green becoming dark brown; not aware of this, the copyist has not only imitated the altered colors, but added a little to their intensity; the consequence of which is, that the figures thence obtained in the expensive work referred to, are made to appear in black, when they should only be a pale grey or green." p. 355, *note.* Mortified to find, that a departed naturalist had thus been robbed of a portion of his well earned fame, and proud to be the means of rescuing from oblivion the labors of Buchanan, Dr. M'Clelland has not only in every instance, when practicable, referred to his plates in the most generous manner, but has even presented us with some of the original plates, although he had made similar drawings previous to their discovery. His whole conduct in this transaction is noble and disinterested; and while the naturalist, after reading this volume, acknowledges his obligations for the information received, he will also feel for the author a deep and abiding esteem.

ART. XI.—*Des Moulins' General Considerations on Restricting the number of Species of the Genera Unio and Anodonta;* translated from the French, by PHILIP H. NICKLIN, A. M.

Prefatory Remarks by the Translator.

THE paper of Mr. Charles des Moulins, now offered in an English translation, possesses merits of a very high order. It criticises without acrimony, and praises without flattery; it is scientific without pedantry, and philosophical without obscurity. It is imbued throughout with the enlightened spirit of catholic Christianity, which does honor to the writer, and is a favorable symptom of the moral condition of the scientific world.

Mr. Des Moulins was the predecessor of Dr. Graleloup in the presidency of the Linnæan Society of Bordeaux, and is doubtless a naturalist of a high order. On the presentation of the 2nd volume of Mr. Isaac Lea's Conchological Observations to the Linnæan Society, Mr. Des Moulins requested of the Society to be charged with the '*Rapport*,' on account of his familiar knowledge of the English language, acquired during a long residence in Great Britain. In a letter to Mr. Lea he observes, "as I felt obliged to speak my thoughts plainly in the *rapport*, I ventured to give an explanation of the principles and method on which I would rely, for an enucleation of the *true* species from amidst the mere forms and differences of sex and age, and in short from all sorts of varieties. This theoretical part of my paper, the Linnæan Society have judged fit to be printed in their transactions." "As for the second part of my work, which was plainly the Report on your volume, it was not admissible in such a kind of review as our transactions, where original writings only can be admitted, and it is not worth my sending a manuscript copy. Let me only tell you that I gave a numerical account of the species of each genus you described, that I criticised your changing the Lamarckian generic name *Iridina** for *Platiris*, and the specific one of *Iridina rostrata*, Rang, for *cælestis*, while it remains evi-

* If 'Iridina' had been retained for the genus, it would have been necessary to strike from Lamarck's generic description the following phrase, "*per longitudinem tuberculosus, subcrenatus; tuberculis inæqualibus, crebris;*" because the *Iridina rubens* and its fellows, now forming Mr. Lea's subgenus *Spatha*, do not fulfil that cardinal condition: it was therefore manifestly proper to adopt a new name for the

dent by your own plate, that the shells are exactly the same. I did specially point to your excellent anatomical researches on the animals of Naïades, and to that valuable Synoptical Table, on which you must have toiled so long and so hard, to enubilate the tremendous synonomy of those poor shells. Lastly, after some other analytical details, I ended my report as follows:

"Tel est Messieurs, le bel ouvrage dont votre bibliothèque vient d'être enrichie par la générosité d'un des plus laborieux de vos correspondans. Favorisé par toutes les circonstances qui facilitent et encouragent les travaux du naturaliste, Mr. Lea vous donne l'espoir de voir les siens se multipliér chaque année, ainsi que ses titres à la reconnaissance des savans. Témoignez-lui la vôtre, Messieurs, par l'organe de M. le Secretaire Général, car elle est bien méritée, et je le demande comme justice, a la Société Linnéenne, en terminant ce rapport."

The paper of Mr. Des Moulins, though critical, is highly honorable to the labors of our countrymen, shewing that they are casting their mites into the general treasury of the world's knowledge. The tribe of the Naïades is one of the most interesting of Nature's families, dwelling in cells of pearl, adorned within, with all the hues of flowers and brilliant rainbow dyes, inhabiting every crystal lake and running stream of this terraqueous globe. Man doth press with wandering foot no river bank, nor margin of translucent lake, but there a lovely Naïs doth wait and woo his admiration;

—"candida Naïs
Pallentes violas et summa papavera" *ferens.—Virg. Ecl. 2, l. 46.*

This is more abundantly the case in our own youthful country, which, as Mr. Des Moulins justly observes, is "évidemment la patrie privilégiée des Unios et des Anodontes;" it is therefore not surprising that our young naturalists should have directed their researches with great enthusiasm towards that interesting branch of natural history, in the beautiful subjects of which our country abounds so much more than all other parts of the world. Mr. Des Moulins arrives at a just conclusion, when he says, "Il vaut probablement mieux lâcher encore les rênes, pendant quel-

genus, and to use Lamarck's name *Iridina* for the *subgenus*, in which are arranged all the species which conform to his generic description. It would not have been expedient to make two genera, because the animals of *Iridina* and *Spatha* are supposed to be nearly identical.—*Translator's Note.*

ques années, aux collecteurs, aux observateurs, aux faiseurs d'espèces même, pour ne pas les decourager; et pour laisser ainsi s'augmenter notablement la masse des matériaux;" so that, when the heap of knowledge shall be thus "*notablement augmentée*," some future classifier, endowed perhaps with a Linnæan power of generalizing, may extinguish the superfluous species, and arrange the remainder in exact scientific order.

Mr. Des Moulins wears his Christian mantle with so much grace and courage, that he has even quoted Saint Augustine for a profound generalization of the works of God. And perhaps in this matter the saint has excelled the *savant;* for the latter offers the following exposition of the three kingdoms:

Mineralia crescunt,
Vegetabilia crescunt et vivunt,
Animalia crescunt et vivunt et sentiunt;

but the former says:

Quædam sunt, i. e. Mineralia;
Quædam sunt et vivunt, i. e. Vegetabilia;
Quædam sunt, vivunt et sentiunt, i. e. Animalia.

Mr. Des Moulins seems to be in error in making *growth* a predicate of minerals. He applauds Linnæus for having borrowed "*presque textuellement à Saint Augustin les trois premiers termes de cette magnifique progression d'institution divine*," and yet himself departs from the Augustinian text by substituting *crescere* for *esse;* and as he departs, so far he errs.

The Linnæan scheme is as follows:

Lapides corpora *congesta*, nec viva, nec sentientia.

Vegetabilia corpora *organisata et viva*, non sentientia.

Animalia corpora *organisata et viva et sentientia*, sponteque se moventia.

Minerals are bodies *aggregate*, not living nor perceiving.

Vegetables are bodies *organic and living*, not perceiving.

Animals are bodies, *organic, living and perceiving*, and moving of their own accord.

The schemes of the two modern philosophers, when compared with that of the ancient divine, seem to be deficient in severity and precision.

Young naturalists will find Mr. Des Moulins' paper an interesting and improving study; and the application of its principles need not be restrained to the branch of natural history in discus-

sion in its pages. The important question, "*what is species?*" which he asks but does not answer, seems to present itself at the beginning of every arrangement; and that naturalist would be canonized who should answer it in a manner acceptable to all.

The definition given by Milne Edwards in his Elémens de Zoologie, seems to be sufficient for the purpose of arrangement: "On donne le nom d'espèce à la réunion des individus qui se reproduisent entre eux avec les mêmes propriétés essentielles."

P. H. N.

Philadelphia, April 10, 1841.

General Considerations on the Reform which ought to take place in restricting the number of Species of the genera Unio and Anodonta. Presented to the Linnæan Society of Bordeaux, at its sitting of the 5th of July, 1839, on the occasion of the publication of Mr. Isaac Lea's second volume of Conchological Observations. By Ch. Des Moulins, Member of the Linnæan Society of Bordeaux, &c.

Gentlemen—An able designer had arranged, in a hundred adjoining compartments, a hundred figures, of which each one differed so little in appearance from its neighbor, as to be indistinguishable at the first glance. The first was the figure of the Apollo Belvidere, the last was that of a frog. This curious comparison shadows forth a relation little flattering to the physical man, but of much importance in a *philosophical** point of view, in the sense now attached to this word. It is the graphic representation of the celebrated maxim of Linnæus, *Natura non facit saltum*—'Nature makes no leaps.'

But really, gentlemen, what is nature? Nature, according to the notion of Linnæus, in the opinion of Christians, in truth is nothing else than the *whole of those laws*, or *material order*, which God imposed on physical things when he created them. Now gentlemen, this saying of Linnæus is admirable, it is profound, it is true in the *material order*, the object at once of the study and teaching of the Professor of Upsal; and the more worthy it is of our respect, the greater is the necessity of adding to it a commentary, restraining its scope within the bounds contemplated by its immortal author.

* The use of this word often appears to me grammatically absurd; sometimes, instead of it, we ought to say *physiological*, but oftener *metaphysical*; but unfortunately the latter frightens children, even children of all ages.

This saying is true; observe its justness from kingdom to kingdom; behold the close connexion of the mineral with the vegetable kingdom, in those innumerable crystals which form the microscopic quasi skeleton of the stem of the Cactæa, and of so many other plants; behold it also, though in a different form, in those *Chara*, so delicate as to be always protected by a stony covering; observe the relation between the mineral and the animal kingdoms, in those curious madrepores, of which the frame-work, entirely mineral, is secreted by a feeble living jelly; behold, finally, that of the animal with the vegetable kingdom, in those corals which unite the essential composition of the former with the flexibility and the appearance of the laws of growth of the latter: *Natura non facit saltum.*

Let us proceed, and this maxim will shed more light. It is not a simple line of junction that marks the boundary between the inorganic and the two organic kingdoms; they go even to a superposition, or if I may invent a term to express my idea, to a *superaddition*, so constant as to form one of the essential laws of the physical world. Nothing is purely animal, nothing is purely vegetable, since every organic substance is reduced, by analysis, to inorganic elements. The mineral or inorganic kingdom is, then, the primary matter of the physical world—matter which its Creator has set in action and modified by *superadding* other principles entirely foreign to it: *Natura non facit saltum.*

But observe, gentlemen, this wonderful concatenation: the inorganic kingdom, which enters into the composition of all physical things, is subject to *all universal* laws, and is limited by *one only* of those which govern more *complicated creatures:* MINERALIA CRESCUNT; behold the basis of the physical world. God works, and *superadds* the principle of vegetable life, hidden from our feeble intellect: VEGETABILIA CRESCUNT ET VIVUNT. God works again, but in another direction, and *superadds* the principle still more mysterious and sublime, of animal life, of sensation, of volition: ANIMALIA CRESCUNT, VIVUNT ET SENTIENT. Here you still perceive, *Natura non facit saltum.*

The naturalist stops here; he stops where stopt the mind of the great classifier of material things, at the bound which necessarily limited the extent of his immortal maxim. Linnæus, after having borrowed, almost in very phrase from Saint Augustine, the first three terms of this divinely instituted magnificent pro-

gression, stops before the fourth term,* which did not concern him at that moment, or rather he transfers to another place its expression, and guided by his sublime genius, and still more by the bias of his religious heart, places it in the specific name which he has given to man: HOMO SAPIENS.

And now, gentlemen, behold the curtain is raised; for in this double name is fully expressed the double substance of the privileged being. By one of his two essences, he stands on the sum-

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* Quædam enim tangis† ut sint, non tamen ut vivant, sentiant et discernant; quædam verò tangis ut sint et vivant, non tamen ut sentiant et discernant; quædam verò tangis ut sint, vivant et sentiant, non tamen ut discernant; quædam verò tangis ut sint, vivant, sentiant et discernant.—AUGUST. *Medit. Cap.* 29, § 3.

Some things thou commandest *to be*, but not to live, perceive, and understand; some thou commandest *to be and live*, but not to perceive and understand; some thou commandest *to be, live, and perceive*, but not to understand; and some thou commandest *to be, live, perceive, and understand.*

And lest it should be supposed that such a passage could flow from his pen unintentionally, and without sufficient reflection and long meditation, I quote here what Saint Augustine says upon the same subject in another of his works.

Non eram, et creasti me; nihil fueram, et de nihilo me aliquid fecisti. Quale autem aliquid? Non stillam aquæ, non ignem, non avem vel piscem, non serpentem, vel aliquid ex brutis animalibus; non lapidem vel lignum, non ex eorum genere quæ tantum habent esse, vel ex eorum quæ tantum esse possunt, et crescere; non ex eorum genere quæ tantum esse et crescere et sentire possunt; sed super omnia hæc, voluisti me esse ex his quæ habent esse, quia sum; et ex his quæ habent esse et crescere, quia sum et cresco; et ex his quæ sunt, crescunt et sentiunt, quia sum, cresco et sentio: et paulò minùs parem me creasti angelis, quia rationem te cognoscendi, cum ipsis à te communem accepi.—AUGUST. *Soliloq. Cap.* 7, § 4.

I was not, and thou createdst me; I had been nothing, and of nothing thou madest me something. But what? not a drop of water, not fire, not a bird, nor a fish, not a serpent, nor any irrational animal; not a stone, nor a stock, nor any of that kind which can only *be*, nor of that which can only *be* and *grow;* nor of that which can only *be*, and *grow*, and *perceive;* but above all these, thou hast willed me to be of those which can *be*, because I *am;* and of those which can *be* and *grow*, for I *am* and *do grow;* and of those which can *be*, *grow*, *and perceive*, for I *am*, *do grow*, *and do perceive;* and thou hast created me a little lower than the angels, because in common with them I have received reason, by which I am able to know thee.

Saint Augustine is one of the most astonishing phenomena in the history of the human mind. With what power of observation and reasoning must this African bishop have been gifted, who being neither evangelist nor prophet, but merely a doctor of the church, yet aided by the Bible, advanced fourteen hundred years beyond his age in scientific discovery!

† I have not rendered the verb *tangere* by its common English equivalent *to touch*, because it is plain that things must first be created before they can be touched, and because the Bible informs us that God created all things by his word. Saint Augustine uses it to express the action of creative power.—*Translator's note.*

mit of the ladder whose descending steps degrade him to a frog; and by a further descent even to a monad: by the other, he is placed on the lowest rung of the sublime ladder of created intelligences. But, let us speak to our senses: there is a point, in the hundred compartments of which I first spoke, where the *homo sapiens* finishes, a point where the *brute* begins; there is the impracticable *leap*, *the gulf* without a bridge, which separates the immaterial from the physical creation: there is the immense, the impenetrable abyss, which the hand of the Almighty has left between the Apollo and the frog.

It may seem that these reflections are too high, or too abstruse if you will, to serve as the preamble to a critical examination of certain new species of shells. I do not think so, gentlemen; we can only judge piecemeal; and when the subject is intellectual, the pieces are principles. I thought a full explanation necessary, that you might the better appreciate the elements of the discussion which I propose to submit to your consideration. From what I have stated, I now proceed to draw the single rule that will serve as the basis of my labors.

Between the first and the last of our hundred compartments, there is a point where *the man* ends, where *the brute* begins; a point at which, notwithstanding the imperceptible deterioration of external form, there is a change of nature, a change of material classification, a change of order, a change of class, a change of family, a change of genus, a change of species. This consideration is enough for my purpose; without illustrations, its announcement is here sufficient.

But, gentlemen, the most eminent zoologist now living in France, (M. de Blainville,) stated twelve or fifteen years ago, that in considering the embarrassing variety of forms then known in the genera *Unio* and *Anodonta*, he felt the necessity of enquiring, whether, if the two genera* were joined again into one, as their comparative anatomy requires, *that one* should present to the classifying naturalist, an almost endless series of really distinct species, or only *one species* infinitely variable in its forms.

* In March, 1829, the translator expressed the opinion, "that the seven genera, now referred to the family of *Naiades*, are founded in artificial distinctions, and not in nature; and that in fact the family contains but one genus."—*Trans. of the Amer. Phil. Soc., Vol.* 3, *p.* 398.

I do not think that M. de Blainville, seeing the rich materials since published by Mr. Isaac Lea, would now feel a similar hesitancy. It may be supposed, that in a series of specimens more or less numerous, selected with care from the different domestic and foreign forms, the passage may be almost insensible from the immensely large cardinal teeth of the *Unio crassissima* to the toothless hinge of the *Anodonta cygnea*. But if a hundred specimens be necessary to pass through all these successive gradations; I dare say another series of different specimens quite as numerous, would be required to pass insensibly from the very round form of the *Unio circulus* to the linear form of the *Unio rectus*, exhibiting successively all the modifications of form contained between these extremes; and I warrant that still new series almost as numerous, would be necessary to form a communication between the most solid and stony species of the United States, and the *Unio lævissimus;* which is so thin that atmospheric drought makes it split; or to connect those whose form is almost as compressed as that of certain *Lutrariæ*, with those whose three diameters are nearly equal. In fine, I do not think that a single series, however numerous, can exhibit at once all these combinations, with all these various conditions of gradation. And since it required ninety eight designs, almost entirely imaginary, to make an uninterrupted chain between the Apollo and the frog, what would the number be, when we should find ourselves engaged with nature herself, so rich in resources, so abounding in ornament, so inexhaustible in the variety of forms that she exhibits to our admiration.

But in the same manner, (and even oftener than the ladder of ninety eight steps which separates the Apollo from the frog,) comparative anatomy, (which also offers us a clear scale of perceptible gradations,) discovers to us the successive and gradual changes of *orders*, of *classes*, of *families*, of *genera* and of *species;* and it is enough to cast the eye over a rich collection of *Naïades*, or over the magnificent figures contained in Mr. Lea's two volumes, to perceive there at the first glance, a certain number of typical forms, distinct, and each surrounded by derivative forms, more or less numerous; which are too closely connected with them to be separated from them, or even to form connecting links between them and other types entirely different.

The truth, in this matter, seems to me then, intermediate to two extreme propositions, between which M. de Blainville seemed to pause some years ago. Yes, I can confidently say, that there really exist several species, nay, many species in the great genus formed by the union of the *Anodontæ* with the *Uniones*. But I may add with equal confidence, that authors have too greatly multiplied these species. Does this criticism apply also to our honorable correspondent, Mr. Isaac Lea? in my opinion it does. The more of peculiar importance that I attach to these types, so well defined, so perspicuous, so distinguished by their forms, of which, the American authors above all, and Mr. Lea more than any of them, have discovered to us the existence, the more I am convinced of the necessity of degrading to the rank of varieties, and often to that of deviations, (as our learned colleague, M. Casimir Picard of Abbeville says,) those variations of form typically the same, those specimens from different localities, which are not identical in the detail of parts, which are neither precisely this, nor exactly that, but which possess a clear and undeniable relationship with such or such a type perspicuously defined. I think that a certain number of Mr. Lea's species would be extinguished by a reform based upon this principle.

But to propose this reform in detail, we should have under our eyes the collection of Mr. Lea itself, probably the richest in the world; or at least a collection very numerous in species and specimens, identified by comparison with all the published figures of authors. To establish such reform irrevocably, still more is necessary; we must have what at this moment may be considered as impossible, *the comparative anatomy of all the pretended species* established by authors. Then only could we be absolutely sure of viewing the question in all its phases. I think I may affirm that we should then find anatomical differences between all true species, and we would rank without scruple among *forms* or *varieties* all those diversities as to length, shortness, &c. which exert no influence upon the internal structure, the number, or the functions of the organs and integuments. Thus, better than in any other way, should we approach the solution of that important question, *what is species?* a question which so much occupies, and still more divides philosophers. Every body demands a definition of species, but a definition *assented to by all.* Definitions, indeed, are not wanting; but common assent sanctioning

one exclusively, is entirely wanting, and will perhaps always be so, if I am right in classing among *impossibilities*, the only means of bringing about such assent.

What is to be done, then, in the midst of all these difficulties? How shall we escape the horns of this dilemma; *reform is wanted, but permanent reform is at this time impossible;* nevertheless, on the other hand, *it is absolutely necessary to classify the objects already known, and those which are discovered every day.* There is nothing but provisional arrangement, nothing but system, (and *system* is *artificial* method,) which can rid us of this embarrassment. Let us, then, consult theory, experience, analogy, that we may labor to come at the truth.

Theory says, that there are essential differences between genus and genus, between species and species.

Experience tells us, that dissimilar animals are sometimes found in similar shells, and animals almost identical in shells, apparently very different.

Analogy proposes, under these premises, to draw inductions from the mass of discoveries already made.

Let us take then this immense group, such as MM. de Blainville and Deshayes understood it a few years ago; this group, whose animals they then believed to be *generically identical;* let us begin by lopping off, finally, with M. Deshayes, the *Iridinæ*, with M. D'Orbigny the *Castaliæ*, of which the shell alone did not permit a rational distinction. Then let us reduce, with Mr. Lea, all the other genera of the *Naïades*,* to the simple importance of subgenera or sections, waiting always to withdraw from these, all those species whose anatomical characters when known, shall be found to differ from those of the studied type, in like manner as the *Iridinæ* and *Castaliæ*, and we shall have a genus which will possess only a *supposed unity* in its essential characters, but which on that very account will be conditionally, and therefore theoretically, exact.

Arriving at species, we would proclaim as irrevocably autonomous, those which offered no specific anatomical differences, such as the form of the ovaries studied by Mr. Lea in the *Unio irroratus, ochraceus, cariosus, retusus*, in the *Anodonta undulata* and

* I say nothing here of the genus *Mycetopoda* of M. d'Orbigny, which I have not at present an opportunity of studying. Mr. Lea does not admit it, any more than that of *Castalia*.

fluviatilis; such as the form of the foot or of the appendices of the mantle in the *Unio radiatus*, &c.

Secondly, we will take for our guide that very important publication which Mr. Lea is at present making of observations made by Dr. Kirtland and himself on the dioïcity of the Naïades, and on the variations of form which it produces in individuals of the same species, and we should thus arrive at the certain and rational extinguishment of many species that authors have established, as Mr. Lea admits, solely upon their external character.

Thirdly, we will apply most rigorously the luminous and solid principles that M. Casimir Picard, of Abbeville, is going to publish, I hope, in a short time, upon the *deviations* or *pathological malformations* of the *Naïades*; and many more species would disappear in the sieve of this physiological scrutiny.

Fourthly, we will examine the laws of the geographical distribution of animals in the different classes, and, governed by analogy, we will not admit cosmopolitan species, except with the greatest reserve; but we will take equal care not to give too much specific importance to a difference of localities or of *habitat* within the limits of the same *zoological region*.

By means of these four considerations, we shall have exhausted all that *experience* and *analogy* offer as regards theory; and it will remain for us,

Fifthly, to resort to artificial method to finish conditionally, with all proper reservations, this great work of reform.

Here we shall be helped in the choice of types by the aid of consideration and combination; 1st, of the general form regarded as the generatrix of the modifications which will constitute simple varieties; 2dly, of the pallial and muscular impressions, and of the ligament; 3dly, of the general system of structure of the appendices of the shell, whether *null*, *pliciform*, *nodulous*, *spinous*, *symphynote* or *not*, (these ingenious divisions are due to Mr. Lea;) 4thly, of the essential and constant character of the structure of the hinge; 5thly, of the thickness of the shell; 6thly and finally, of the general system of coloring; for we must, as Mr. Lea has done in many cases, deny *specific* value to epidermal coloring, and still more to rays so variable in form, in magnitude, and in the different ages, and especially to the superficial coloring of the nacre.

I shall cite as examples of this mode of judging, the entirely peculiar system of coloring of the *Unio irroratus* and *cylindricus;* the unique form of the appendices in the *Unio spinosus* and in the *Symphynota bialata* (Lea, 1st volume;) the peculiar form of the tubercles in the *Unio lacrymosus* and *apiculatus*, &c.

By combining all these means of exact judgment on the one part, and of logical induction on the other, I am satisfied that we shall recognize in the group of the Naïades, as experience has taught us in others, the truth of that observation, whose expression is recorded in the maxim, *Natura non facit saltum*, viz. that exterior unlikeness does not exclude autonomy, and *vice versâ;* or, in other words, that two beings inseparably connected by their essential characters, are often much more unlike in their external appearance, than two beings entirely different in essential characters. The demonstration of this proposition is found by the zoologist, in the *races*, so abounding in varieties, of the dog, the ox, the cock, of man himself compared with the *real* species but little removed, of the quadrumana, the rodentia, the motacillæ, the accipitres, etc. The botanist finds it, in comparing the *races* so prodigiously varied of the pears, the roses, the carnations, of the cereal plants and pot-herbs, with the true species, although differing little in appearance, of the genera *Ranunculus*, *Galium*, *Artemisia*, *Senecio*, *Carex*, *Bromus*, etc. The mineralogist and the geologist find it, as every one knows, at every step of their investigations.

Such, gentlemen, are the principles, upon which, (in default of absolute certainty, perhaps not attainable,) I would reform the demarcation of the species of the family of the Naïades. And I must say, that no philosopher seems to me at the same time more able, and better situated to accomplish it than Mr. Lea himself. But has the time arrived in which he can thus occupy himself usefully? I think not yet. Possessing the richest of *special* collections, Mr. Lea has not yet, however, had the opportunity of inspecting *all* the nominal species. America, where he lives, is evidently the privileged country of the Uniones and Anodontæ, considered with respect to the number of species; but America has not yet been so entirely explored in this point of view, as not to leave a great number of links wanting in that chain, (which should be unbroken if possible,) upon which the reformer should mark his divisions. It would probably be better, still to loose the

reins for some years, to collectors, to observers, and even to makers of species, in order to encourage them, and thus induce them to increase greatly the mass of materials. In this latter relation, no author has rendered more eminent services to science than Mr. Lea, in his first volume, which is already in your library, and also in his second, of which I am going to exhibit to you briefly, the contents. CH. DES MOULINS.

ART. XII.—*Description of an American Spider, constituting a new sub-genus, of the tribe Inæquitelæ of Latreille;* by Prof. N. M. HENTZ, Florence, Ala.

[Read before the Yale Natural-History Society, April 28, 1841.]

THE genus Aranea of Linnæus, like most of the genera established by that great man, is now in fact an extensive family of the animal kingdom. Walckenaer and Latreille subdivided it, and at once classified the numerous species known to them, in an admirable order. We may add the species since discovered and such subgenera as were not known to those authors, without materially altering their superstructure. But when the work is accomplished, and all nature is described by man, the number of species included in the common word *spider*, will be truly amazing. Walckenaer enumerated 260 species thirty four years ago, and Latreille could easily have doubled the catalogue, if the number of species had been mentioned in the last edition of the *Règne Animal.* The writer of this paper, in the course of twenty years, has, at stolen hours, collected and described 147 species; but he is convinced that *fifty* more could be added; as he has not explored the vast peninsula of Florida, nor any portion of Louisiana. Two hundred species, therefore, would be a low estimate of the number of spiders inhabiting the United States, not including the territories yet unoccupied by civilized men. It is obvious that the number of species throughout the world will amount to more than *two thousand*, when the natural history of all countries is complete. It is equally obvious that the rapidly increasing number of new species requires subdivisions, when it is practicable to make them. The subgenus now proposed is indispensable, as the species cannot be classed under any existing gen-

eric name. It will be placed in a natural order immediately after PHOLCUS.

Subgenus SPERMOPHORA.

Eyes, six, in two clusters, one on each side of the cephalothorax, thus

Legs, the *first* pair longest, then the *fourth* and *second*, nearly equal, the *third* pair shortest. Length moderate, slender.

Lip, wide, triangular.

Maxillæ, tapering towards the point, inclined over the lip.

Mandibulæ, short, conical, with very small fangs.

The characters derived from the trophi, are nearly those of Pholcus, but the absence of the two eyes in front of the cephalothorax, would alone, remove this spider from that subdivision. Moreover, the legs, which in Pholcus are excessively long, are here of a moderate length. This spider, which is wholly of a pale hue, makes its very loose web in dark places, under rubbish. The female carries in its mandibles its eggs glued together without any silk, until they are hatched. Inhabits Alabama.

This species, the 137th of my MS. catalogue, is there named *Spermophora meridionalis*. Of the 147 species comprised in this catalogue, there are not ten mentioned in European works, besides those described by Bosc, whose manuscript was never printed.*

Florence, Ala., September 2, 1839.

ART. XIII.—*Contributions to Electricity and Magnetism.—No. IV. On Electro-Dynamic Induction;* by JOSEPH HENRY, LL. D., Professor of Natural Philosophy in the College of New Jersey, Princeton. Read June 19, 1840.†

(Continued from p. 243, Vol. XXXVIII, No. 2, of this Journal.)

INTRODUCTION.

1. In the course of my last paper, it was stated that the investigations which it detailed were not as complete in some parts as I could wish, and that I hoped to develop them more fully in

* A valuable catalogue and description by Prof. Hentz, of the genera of Araneides found in this country, was published in this Journal, Vol. XXI, p. 100—109, 1832.—*Eds.*

† From the Transactions of the American Philosophical Society, Vol. 8, N. S.

another communication. After considerable delay, occasioned by alterations in the rooms of the physical department of the college, I was enabled to resume my researches, and since then I have been so fortunate as to discover a series of new facts belonging to different parts of the general subject of my contributions. These I have announced to the Society at different times, as they were discovered, and I now purpose to select from the whole such portions as relate particularly to the principal subject of my last paper, namely, the induction at the beginning and ending of a galvanic current, and to present them as a continuation, and, in a measure, as the completion, of this part of my researches. The other results of my labors in this line will be arranged for publication as soon as my duties will permit me to give them a more careful examination.

2. In the course of the experiments I am about to describe, I have had occasion to repeat and vary those given in my last paper, and I am happy to be able to state, in reference to the results, that, except in some minor particulars, which will be mentioned in the course of this paper, I have found no cause to desire a change in the accounts before published. My views, however, of the connection of the phenomena have been considerably modified, and I think rendered much more definite by the additional light which the new facts have afforded.

3. The principal articles of apparatus used in these experiments are nearly the same as those described in my last paper, namely, several flat coils and a number of long wire helices. (III, 6, 7, 8)* I have, however, added to these a constant battery, on Professor Daniell's plan, the performance of which has fully answered my expectations, and confirmed the accounts given of this form of the instrument by its author. It consists of thirty elements, formed of as many copper cylinders, open at the bottom, each five inches and a half in height, three inches and a half in diameter, and placed in earthen cups. A zinc rod is suspended in each of these, of the same length as the cylinders, and about one inch in diameter. The several elements are connected by a thick copper wire, soldered to the copper cylinder of one element, and dipping into a cup of mercury on the zinc of the next. The cop-

* When the numerals II or III are included in the parenthesis, reference is made to the corresponding Nos. of my contributions.

per and zinc as usual are separated by a membrane, on both sides of which is placed a solution of one part of sulphuric acid in ten parts of water; and to this is added, on the side next the copper, as much sulphate of copper, as will saturate the solution. The battery was sometimes used as a single series, with all its elements placed consecutively, and at others in two or three series, arranged collaterally, so as to vary the quantity and intensity of the electricity as the occasion might require.

4. The galvanometers mentioned in this paper, and referred to in the last, are of two kinds; one, which is used with a helix, to indicate the action of an induced current of intensity, consists of about five hundred turns of fine copper wire, covered with cotton thread, and more effectually insulated by steeping the instrument in melted cement, which was drawn into the spaces between the spires by capillary attraction. The other galvanometer is formed of about forty turns of a shorter and thicker wire, and is always used to indicate an induced current, of considerable quantity, but of feeble intensity. The needle of both these instruments is suspended by a single fibre of raw silk.

5. I should also state, that in all cases where a magnetizing spiral is mentioned in connection with a helix, the article is formed of a long, fine wire, making about one hundred turns around the axis of a hollow piece of straw, of about two inches and a half long: also the spiral mentioned in connection with a coil, is formed of a short wire, which makes about twenty turns around a similar piece of straw. The reason of the use of the two instruments in these two cases is the same as that for the galvanometers, under similar circumstances, namely, the helix gives a current of intensity, but of small quantity, while the coil produces one of considerable quantity, but of feeble intensity.

Section I.

On the Induction produced at the moment of the Beginning of a Galvanic Current, &c.

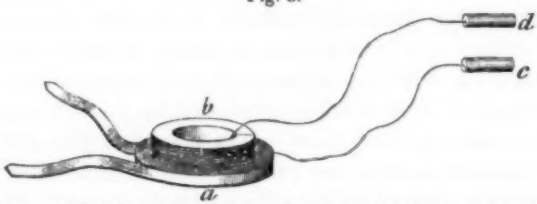
6. It will be recollected that the arrangement of apparatus employed in my last series of experiments gave a powerful induction at the moment of breaking the galvanic circuit, but the effect at making the same was so feeble as scarcely to be perceptible. I was unable in any case to get indications of currents of the third

or fourth orders from the beginning induction, and its action was therefore supposed to be so feeble as not materially to affect the results obtained.

7. Subsequent reflection, however, led me to conclude, that in order to complete this part of my investigations, a more careful study of the induction at the beginning of the current would be desirable, and accordingly, on resuming the experiments, my attention was first directed to the discovery of some means by which the intensity of this induction might be increased. After some preliminary experiments, it appeared probable that the desired result could be obtained by using a compound galvanic battery, instead of the single one before employed. In reference to this conjecture the constant battery before mentioned (3) was constructed, and a series of experiments instituted with it, the results of which agreed with my anticipation.

8. In the first experiment, coil No. 2, which it will be remembered (III, 7) consists of a copper riband of about sixty feet long, and coiled on itself like the main spring of a watch, was connected with the compound battery, and helix No. 1, (III. 8,) formed of one thousand six hundred and sixty yards of fine copper wire,

Fig. 3.



a represents coil No. 1, *b* helix No. 1, and *c*, *d*, handles for receiving the shock.

was placed on the coil to receive the induction, as is shown in figure 3, which is again inserted here for the convenience of the reader. This arrangement being made, currents of increasing intensity were passed through the coil, by constantly retaining one of its ends in the cup of mercury forming one extremity of the battery, and successively plunging the other end into the cups which served to form the connections of the several elements of the battery. With the current from one element, the shock at breaking the circuit was quite severe, but at making the same it was very feeble, and could be perceived in the fingers only or through the tongue. With two elements in the circuit, the shock

at beginning was slightly increased; with three elements the increase was more decided, while the shock at breaking the circuit remained nearly of the same intensity as at first, or was comparatively but little increased. When the number of elements was increased to *ten*, the shock at making contact was found fully equal to that at breaking, and by employing a still greater number, the former was decidedly stronger than the latter, the difference continually increasing until all the thirty elements were introduced into the circuit.

9. In my last paper, a few experiments are mentioned as being made with a compound battery of Cruickshank's construction; but from the smallness of the plates of this, and the rapidity with which its power declined, I was led into the error of supposing that the induction at the ending of the current, in the case of a short coil, was diminished by increasing the intensity of the battery, (see paragraph 19, of No. III,) but by employing the more perfect instrument of Professor Daniell in the arrangement of the last experiment, I am enabled to correct this error, and to state that the induction at the ending remains nearly the same, when the intensity of the battery is increased. If the induction depends in any degree on the quantity of current electricity in the conductor, then a slight increase in the induction should take place, since, according to theory, the current is somewhat increased in quantity, in the case of a long coil, by the increase of the intensity of the battery. Although very little, if any, difference could be observed in the intensity of the shock from the secondary current, yet the snap and deflagration of the mercury appeared to be greater from the primary current, when *ten* elements of the battery were included in the circuit, than with a single one. The other results which are mentioned in my last paper in reference to the compound battery are, I believe, correctly given.

10. The intensity of the different shocks in the foregoing experiments was compared by gradually raising the helix from the coil, (see Fig. 3,) until, on account of the distance of the conductors, the shock in one case would be so much reduced as to be scarcely perceptible through the fingers or the tongue, while the shock from another arrangement, but with the same distance of the conductors, would be evident, perhaps, in the hands. The same method was generally employed in the experiments in which

shocks are mentioned as being compared, in the other parts of this paper.

11. Experiments were next made to determine the influence of a variation in the length of the coil, the intensity of the battery remaining the same. For this purpose, the battery consisting of a single element, and the arrangement of the apparatus as represented in Fig. 3, the coil was diminished in length from sixty feet to forty five, then to thirty, and so on. With the first mentioned length the shock, at making contact with the battery, was, of course, very feeble, and could be felt only in the tongue; with the next shorter length it was more perceptible, and increased in intensity with each diminution of the coil, until a length of about fifteen feet appeared to give a maximum result.

12. The diminution of the intensity of the shock in the last experiment, after the length of the coil was diminished below fifteen feet, was due to the diminution of the number of spires of the coil, each of which, by acting on the helix, tends to increase the intensity of the secondary current, unless the combined length of the whole is too great for the intensity of the battery. That this is the fact is shown by the following experiment: the helix was placed on a single spire or turn of the coil, and the length of the other part of the copper riband, which did not act on the helix, was continually shortened, until the whole of it was excluded from the circuit; in this case the intensity of the shock at the beginning was constantly increased. We may therefore state generally, that at the beginning of the battery current, the induction of a unit of its length, is increased by every diminution of the length of the conductor.

13. In the experiment given in paragraph 11, the intensity of the shock at the *ending* of the battery current diminishes with each diminution of the length of the coil; and this is also due to the decrease of the number of the spires of the coil, as is evident from an experiment similar to the last, in which the helix was placed on a coil consisting of only two turns or spires of copper riband; the shock at the ending, with this arrangement, was comparatively feeble, but could be felt in the hands. Different lengths of coil No. 2 were now introduced into the same circuit, but not so as to act on the helix; but although these were varied from four or five feet to the whole length of the coil, (sixty feet,) not the least difference in the intensity of the shock could be per-

ceived. We have, therefore, the remarkable result, that the intensity of the ending induction of each unit of length of the battery current is not materially altered, at least within certain limits, by changing the length of the whole conductor. From this we would infer that the shock depends more on the intensity of the action than on the quantity of the current, since we know that the latter is diminished in a given unit of the conductor by increasing the length of the whole.

14. We have seen (8) that with a circuit composed of ten elements of the compound battery and the coil No. 2, the shock, at the beginning of the current, was fully equal to that at the ending. It was, however, found that if, in this case, the length of the coil was increased, this shock was diminished; and we may state, as an inference from several experiments, that however great may be the intensity of the electricity from the battery, the shock at the beginning may be so reduced by a sufficient increase of the length of the primary circuit, as to be scarcely perceptible.

15. It was also found that when the thickness of the coil was increased, the length and intensity of the circuit remaining the same, the shock at the beginning of the battery current, was somewhat increased. This result was produced by using a double coil; the electricity was made to pass through one strand, and immediately afterwards through both: the shock from the helix in the latter case was apparently the greater.

16. By the foregoing results we are evidently furnished with two methods of increasing, at pleasure, the intensity of the induction at the beginning of a battery current, the one consisting in increasing the intensity of the source of the electricity, and the other in diminishing the resistance to conduction of the circuit while the intensity remains the same.

17. The explanation of the effects which we have given, relative to the induction at the beginning, is apparently not difficult. The resistance to conduction in the case of a long conductor and a battery of a single element is so great that the full development of the primary current may be supposed not to take place with sufficient rapidity to produce the instantaneous action on which the shock from the secondary current would seem to depend. But when a battery of a number of elements is employed, the poles of this, previous to the moment of completing the cir-

cuit, are in a state of electrical tension; and therefore the discharge through the conductor may be supposed to be more sudden, and hence an induction of more intensity is produced.

18. That the shock at both making and breaking the circuit in some way depends on the rapidity of formation and diminution of the current is shown by the following experiment, in which the tension just mentioned does not take place, and in which, also, the current appears to diminish more slowly. The two ends of the coil were placed in the two cups which formed the poles of the battery, and permanently retained there during the experiment; also, at the distance of about six inches from say the right hand end of the coil, a loop was made in the riband, which could be plunged into the cup containing the left hand end. With this arrangement, and while only the two extreme ends of the coil were in connexion with the cups of mercury, of course the current passed through the entire length of the riband of the coil, but by plunging the loop into the left hand cup, the whole length of the coil, except the six inches before mentioned, was excluded from the battery circuit. And again, when the loop was lifted out of the cup, the whole length was included. In this way the current in the coil could be suddenly formed and interrupted, while the poles of the battery were continually joined by a conductor, but no shock with either a single or a compound battery could be obtained by this method of operation.

19. The feebleness of the shock at the beginning of the current, with a single battery and a long coil, is not entirely owing to the cause we have stated, (17,) namely, the resistance to conduction offered by the long conductor, but also depends, in a considerable degre, if not principally, on the adverse influence of the secondary current, induced in the primary conductor itself, as is shown by the result of the following experiment. Helix No. 1 was placed on a coil consisting of only three spires or turns of copper riband; with this, the shock both at making and breaking the circuit with a single battery could be felt in the hands. A compound coil was then formed of the copper ribands of coils No. 3 and 4 rolled together so that the several spires of the two alternated with each other, and when this was introduced into the circuit so as not to act on the helix by its induction, and the battery current passed through, for example, coil No. 3, the shock at making contact with the pole of the battery was so much redu-

ced as to be imperceptible in the hands, while the shock at breaking the contact was about the same as before this addition was made to the length of the circuit. The ends of coil No. 4 were now joined so as to produce a closed circuit, the induced current in which would neutralize the secondary current in the battery conductor itself; and now the shock at making the contact was nearly as powerful as in the case where the short conductor alone formed the circuit with the battery. Hence, the principal cause of the feebleness of the effect at the beginning of the battery current is the adverse action on the helix of the secondary current produced in the conductor of the battery circuit itself. The shock at the breaking of the circuit, in this experiment, did not appear affected by joining or separating the ends of coil No. 4.

20. Having investigated the conditions on which the inductive action at the beginning of a battery current depends, experiments were next instituted to determine the nature of the effects produced by this induction; and first, the coils were arranged in the manner described in my last paper, (III, 79,) for producing currents of the different orders. The result with this arrangement was similar to that which I have described in reference to the ending induction, namely, currents of the third, fourth, and fifth orders were readily obtained.

21. Also, when an arrangement of apparatus was made similar to that described in paragraph 87 of my last paper, it was found that a current of intensity could be induced from one of quantity, and the converse.

22. Likewise, the same screening or rather neutralizing effect was produced, when a plate of metal was interposed between two consecutive conductors of the series of currents, as was described (III, section 4,) in reference to the ending induction. In short, the series of induced currents produced at the beginning of the primary current appeared to possess all the properties belonging to those of the induction at the ending of the same current.

23. I may mention in this place, that I have found, in the course of these experiments, that the neutralizing power of a plate of metal depends, in some measure, on its superficial extent. Thus a broad plate which extends, in every direction, beyond the helix and coil, produces a more perfect screening than one of the same metal and of the same thickness, but of a diameter only a little greater than that of the coil.

24. The next step in the investigation was to determine the direction of the currents of the different orders produced by the beginning induction, and for this purpose the magnetizing spirals (5) were used, and the results obtained by these verified by the indication of the galvanometer. It should be stated here, as a fact which was afterward found of some importance, that although the needle of the galvanometer was powerfully deflected when the instrument was placed in the circuit of the secondary current, yet a very feeble effect was produced on it by the action of a current of the third, fourth, or fifth order. The directions, however, of these currents, as indicated by the feeble motion of the needle, were the same as those given by the magnetizing spiral.

25. The direction of the different currents produced at the making of the battery current, as determined by these instruments, is as follows, viz. the direction of the secondary current is, as stated by Dr. Faraday, adverse to that of the primary current, and also, the direction of each succeeding current is opposite to that of the one which produced it. We have, therefore, from these results, and those formerly obtained, (III, 92,) the following series of directions of currents, one produced at the moment of beginning and the other at that of the ending of the battery current.

	At the beginning.	At the ending.
Primary current,	+	+
Secondary current,	−	+
Current of the third order,	+	−
Current of the fourth order,	−	+
Current of the fifth order,	+	−

26. These two series, at first sight, may appear very different, but, with a little attention, they will be seen to be of the same nature. If we allow that the induction at the ending of a galvanic battery should be opposite to that at the beginning of the same, then the sign at the top of the second column may be called minus instead of plus, and we shall have the second series − + − + alternating precisely like the first.

27. In connexion with the results given in the last two paragraphs, it is due to Mr. Sturgeon that I should state that, in a letter addressed to me, and published in the Annals of Electricity, he has predicted, from his theory, that I would find on examination, the series of alternation of currents for the beginning induction which I have here given. I may however add, that

it appears to me that this result might have been predicted without reference to any theory. There was no reason to suppose the induction at the beginning would be different in its nature from that at the ending, and therefore the series which would be produced from the former might be immediately inferred from that belonging to the latter, by recollecting that the direction of the induction at the beginning should be opposite to that at the ending. I do not wish it to be supposed, however, from this remark, that I had, myself, drawn any inference from my experiments as to the alternations of currents which might be produced by the beginning induction; the truth is, that this action was so feeble with the arrangement of apparatus I employed, that I supposed it could not produce a series of currents of the different orders.

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28. In the course of the experiments given in this section, I have found that a shock can be produced without using a coil, by arranging about ten elements of the battery in the form of a circle, and placing the helix within this. The shock was felt in the hands at the moment of closing the circuit, but the effect at opening the same was scarcely perceptible through the tongue. An attempt was also made to get indications of induction by placing the helix within a circle of dilute acid, connected with a battery instead of a coil, but the effect, if any, was very feeble.

29. I have shown, in the second number of my contributions, that if the body be introduced into a circuit with a battery of one hundred and twenty elements, without a coil, a thrilling sensation will be felt during the continuance of the current, and a shock will be experienced at the moment of interrupting the current by breaking the circuit at any point. This result is evidently due to the induction of a secondary current in the battery itself, and on this principle the remarkable physiological effects produced by Dr. Ure, on the body of a malefactor, may be explained. The body, in these experiments, was made to form a part of the circuit, with a compound galvanic apparatus, in which a series of interruptions was rapidly made by drawing the end of a conductor over the edges of the plates of the battery. By this operation a series of induced currents must have been produced in the battery itself, the intensity of which was greater than that of the primary current.

30. In this connexion I may mention that the idea has occurred to me that the intense shocks given by the electrical fish may possibly be from a secondary current, and that the great amount of nervous organization found in these animals may serve the purpose of a long conductor.* It appears to me, that in the present state of knowledge, this is the only way in which we can conceive of such intense electricity being produced in organs imperfectly insulated and immersed in a conducting medium. But we have seen that an original current of feeble intensity can induce, in a long wire, a secondary current capable of giving intense shocks, although the several strands of the wire are separated from each other only by a covering of cotton thread. Whatever may be the worth of this suggestion, the secondary current affords the means of imitating the phenomena of the shock from the electrical eel, as described by Dr. Faraday. By immersing the apparatus (Fig. 3,) in a shallow vessel of water, the handles being placed at the two extremities of the diameter of the helix, and the hands plunged into the water parallel to a line joining the two poles, a shock is felt through the arms; but when the contact with the water is made in a line at right angles to the last, only a slight sensation is felt in each hand, but no shock.

31. Since the publication of my last paper, I have exhibited to my class the experiment, (No. III, Sec. 3d,) relative to the induction at a distance on a much larger scale. All my coils were united so as to form a single length of conductor of about four hundred feet, and this was rolled into a ring of five and a half feet in diameter, and suspended vertically against the inside of the large folding doors which separate the laboratory from the lecture room. On the other side of the doors, in the lecture room, and directly opposite the coil, was placed a helix, formed of upwards of a mile of copper wire, one sixteenth of an inch in thickness, and wound into a hoop of four feet in diameter. With this arrangement, and a battery of one hundred and forty seven square feet of zinc surface divided into eight elements, shocks were perceptible in the tongue, when the two conductors were separated, to the distance of nearly seven feet; at the distance of between three and four feet, the shocks were quite severe. The exhibition was

* Since writing the above, I have found that M. Masson has suggested the same idea, in an interesting thesis lately published.

rendered more interesting by causing the induction to take place through a number of persons standing in a row between the two conductors.

Section II.

On apparently two kinds of electro-dynamic induction.

32. The investigations arranged under this head had their origin in the following circumstances. After the publication of my last paper, I received, through the kindness of Dr. Faraday, a copy of the fourteenth series of his researches, and in this I was surprised to find a statement which appeared in direct opposition to one of the principal facts of my communication. In paragraph 59, I state, in substance, that when a plate of metal is interposed between the coil transmitting a galvanic current, and the helix placed above it to receive the induction, the shock from the secondary current is almost perfectly neutralized. Dr. Faraday, in the extension of his new and ingenious views of the agency of the intermediate particles in transmitting induction, was led to make an experiment on the same point, and apparently, under the same circumstances, he found that it "makes not the least difference, whether the intervening space between the two conductors is occupied by such insulating bodies as air, sulphur, and shell-lac, or such conducting bodies as copper and other non-magnetic metals."

33. As the investigation of the fact mentioned above forms an important part of my paper, and is intimately connected with almost all the phenomena subsequently described in the communication, I was, of course, anxious to discover the cause of so remarkable a discrepancy. There could be no doubt of the truth of my results, since a shock from a secondary current which would paralyze the arms was so much reduced by the interposition of plates of metal as scarcely to be felt through the tongue.

34. After some reflection, however, the thought occurred to me that induction might be produced in such a way as not to be affected by the interposition of a plate of metal. To understand this, suppose the end of a magnetic bar placed perpendicularly under the middle of a plate of copper, and a helix suddenly brought down on this; an induced current would be produced in the helix by its motion towards the plate, since the copper, in this

case, could not screen the magnetic influence. Now if we substitute for the magnet a coil through which a galvanic current is passing, the effect should be the same. The experiment was tried by attaching the ends of the helix to a galvanometer,* and the result was as I expected: when the coil was suddenly brought down on the plate, the needle swung in one direction, and when lifted up, in the other; the amount of deflection being the same, whether the plate was interposed or not.

35. It must be observed in this experiment, that the plate was at rest, and consequently did not partake of the induction produced by the motion of the helix. From my previous investigations, I was led to conclude that a different result would follow, were a current also generated in the plate by simultaneously moving it up and down with the helix. This conclusion, however, was not correct, for on making the experiment, I found that the needle was just as much affected when the plate was put in motion with the helix as when the latter alone was moved.

36. This result was so unexpected and remarkable, that it was considered necessary to repeat and vary the experiment in several ways. First, a coil was interposed instead of the plate, but whether the coil was at rest or in motion with the helix, with its ends separated or joined, the effect on the galvanometer was still the same; not the least screening influence could be observed. In reference to the use of the coil in this experiment, it will be recollected that I have found this article to produce a more perfect neutralization than a plate.

37. Next, the apparatus remaining the same, and the helix at rest during the experiment, currents were induced in it by moving the battery attached to the coil up and down in the acid. But in this case, as in the others, the effect on the galvanometer was the same, whether the plate or the coil was interposed or not.

38. The experiment was also tried with magneto-electricity. For this purpose, about forty feet of copper wire, covered with silk, were wound around a short cylinder of stiff paper, and into this was inserted a hollow cylinder of sheet copper, and into this again, a short rod of soft iron; when the latter was rendered

* The arrangement will be readily understood by supposing in Fig. 3, the handles removed, and the ends of the helix joined to the ends of the wire of a galvanometer; also, by a plate of metal interposed between the helix and the coil.

magnetic, by suddenly bringing in contact with its two ends the different poles of two magnets, a current, of course, was generated in the wire, and this, as before, was found to affect the galvanometer to the same degree, when the copper cylinder was interposed, as when nothing but the paper intervened.

39. The last experiment was also varied by wrapping two copper wires of equal length around the middle of the keeper of a horse-shoe magnet, leaving the ends of the inner one projecting, and those of the outer attached to a galvanometer. A current was generated in each by moving the keeper on the ends of the magnet, but the effect on the galvanometer was not in the least diminished by joining the ends of the inner wire.

40. At first sight, it might appear that all these results are at variance with those detailed in my last paper, relative to the effect of interposed coils and plates of metal. But it will be observed that in all the experiments just given, the induced currents are not the same as those described in my last communication. They are all produced by motion, and have an appreciable duration, which continues as long as the motion exists. They are also of low intensity, and thus far I have not been able to get shocks by any arrangement of apparatus from currents of this kind. On the other hand, the currents produced at the moment of *suddenly* making or breaking a galvanic current, are of considerable intensity, and exist but for an instant. From these, and other facts presently to be mentioned, I was led to suppose that there are two kinds of electro-dynamic induction; one of which can be neutralized by the interposition of a metallic plate between the conductors, and the other not.

41. In reference to this surmise, it became important to examine again all the phenomena of induction at suddenly making and breaking a galvanic current.* And in connexion with this part of the subject, I will first mention a fact which was observed in the course of the experiments given in the last section, on the direction of the induced currents of different orders. It was found that though the indications of the galvanometer were the same as those of the spiral, in reference to the direction of the induced currents, yet they were very different in regard to the intensity of the action. Thus, when the arrangement of the apparatus was such that the induction at making the battery circuit was so feeble

* See *Contributions*, No. III.

as not to give the least magnetism to the needle, and so powerful at the ending as to magnetize it to saturation, the indication of the galvanometer was the same in both cases.

42. Also, similar results were obtained in comparing the shock and the deflection of the galvanometer. In one experiment, for example, the shock was so feeble at making contact that it could scarcely be perceived in the fingers, but so powerful at the breaking of the circuit as to be felt in the breast; yet the galvanometer was deflected about thirty-five degrees to the right, at the beginning of the current, and only an equal number of degrees to the left, at the ending of the same.

43. In another experiment, the apparatus being the same as before, the magnetizing spiral and the galvanometer were both at once introduced into the circuit of the helix. A sewing needle being placed in the spiral, and the contact with the battery made, the needle showed no signs of magnetism, although the galvanometer was deflected thirty degrees. The needle being replaced, and the battery circuit broken, it was now found strongly magnetized, while the galvanometer was moved only about as much as before in the opposite direction.

44. Also, effects similar to those described in the last two paragraphs were produced when the apparatus was so arranged as to cause the induction at the beginning of the battery current to predominate. In this case the galvanometer was still equally affected at making and breaking battery contact, or any difference which was observed could be referred to a variation in the power of the battery during the experiment.

45. Another fact of importance belonging to the same class has been mentioned before, (24,) namely, that the action of the currents of the third, fourth, and fifth orders produces a very small effect on the galvanometer, compared with that of the secondary current; and this is not alone on account of the diminishing power of the successive inductions, as will be evident from the following experiment. By raising the helix from the coil, in the arrangement of apparatus for the secondary current, the shock was so diminished as to be inferior to one produced by the arrange ment for a tertiary current, yet, while with the secondary current the needle was deflected twenty-five degrees, with the tertiary it scarcely moved more than one degree, and with the currents of the fourth and fifth orders the deflections were still less, resembling the effect of a slight impulse given to the end of the needle.

46. From the foregoing experiments, I was the more fully persuaded that some new and interesting results might be obtained by a re-examination of my former experiments, on the phenomena of the interposed plate of metal, in the case where the induction was produced by making and breaking the circuit with a cup of mercury; and in this I was not disappointed. The coil, (Fig. 3,) being connected with a battery of ten elements, the shocks, both at making and breaking the circuit, were very severe; and these, as usual, were almost entirely neutralized by the interposition of a zinc plate. But when the galvanometer was introduced into the circuit instead of the body, its indications were the same whether the plate was interposed or not; or, in other words, the galvanometer indicated no screening, while, under the same circumstances, the shocks were neutralized.

47. A similar effect was observed when the galvanometer and the magnetizing spiral were together introduced into the circuit. The interposition of the plate entirely neutralized the magnetizing power of the spiral, in reference to tempered steel, while the deflections of the galvanometer were unaffected.

48. In order to increase the number of facts belonging to this class, the last experiments were varied in several ways; and, first, instead of the hard steel needle, one of soft iron wire was placed in the spiral, with a small quantity of iron filings almost in contact with one of its ends. The plate being interposed, the small particles of iron were attracted by the end of the needle, indicating a feeble, temporary development of magnetism. Hence the current which moves the needle, and is not neutralized by the interposed plate, also feebly magnetizes soft iron, but not hard steel.

49. Again, the arrangement of apparatus being as in paragraph 46, instead of a plate of zinc, one of cast iron, of about the same superficial dimensions, but nearly half an inch thick, was interposed; with this the magnetizing power of the spiral, in reference to tempered steel, was neutralized; and also, the action of the galvanometer was much diminished.

50. Another result was obtained by placing in the circuit of the helix, (Fig. 3d,) at the same time, the galvanometer, the spiral, and a drop of distilled water; with these the magnetizing power of the spiral was the same as without the water, but the deflection of the galvanometer was reduced from ten to about

four degrees. In addition to these, the body was also introduced into the same circuit; the shocks were found very severe, the spiral magnetized needles strongly, but the galvanometer was still less moved than before. The current of low intensity, which deflects the needle of the galvanometer in these instances, was partially intercepted by the imperfect conduction of the water and the body.

51. To exhibit the results of these experiments with still more precision, an arrangement of apparatus was adopted similar to that used by Dr. Faraday, and described in the fourteenth series of his researches, namely, a double galvanometer was formed of two separate wires of equal length and thickness, and wound together on the same frame; and also, a double magnetizing spiral was prepared by winding two equal wires around the same piece of hollow straw. Coil No. 1, connected with the battery, was supported perpendicularly on a table, and coils Nos. 3 and 4 were placed parallel to this, one on each side, to receive the induction, the ends of these being so joined with those of the galvanometer and the spiral that the induced current from the one coil, would pass through the two instruments, in an opposite direction to that of the current from the other coil. The two outside coils were then so adjusted, by moving them to and from the middle coil, that the induced currents perfectly neutralized each other in the two instruments, and the needle of the galvanometer and that in the spiral were both unaffected when the circuit of the battery was made and broken. With this delicate arrangement the slightest difference in the action of the two currents would be rendered perceptible; but when a zinc plate was introduced so as to screen one of the coils, the needle of the galvanometer still remained perfectly stationary, indicating not the least action of the plate, while the needle in the spiral became powerfully magnetic. When, however, a plate of iron was interposed instead of the one of zinc, the needle of the galvanometer was also affected.

52. From the foregoing results it would seem that the secondary current, produced at the moment of suddenly beginning or ending of a galvanic current, by making and breaking contact with a cup of mercury, consists of two parts, which possess different properties. One of these is of low intensity, can be interrupted by a drop of water, does not magnetize hardened steel needles, and is not screened by the interposition of a plate of any

metal, except iron, between the conductors. The other part is of considerable intensity, is not intercepted by a drop of water, develops the magnetism of hardened steel, gives shocks and is screened or neutralized by a closed coil, or a plate of any kind of metal. Also, the induced current produced by moving a conductor towards or from a battery current, and that produced by the movement up and down of a battery in the acid, are of the nature of the first mentioned part, while the currents of the third, fourth, and fifth orders partake almost exclusively of the properties of the second part.

53. The principal facts and conclusions of this section were announced to the Society in October, 1839, and again presented in the form in which they are here detailed in June last. Since then, however, I have had leisure to examine the subject more attentively, and after a careful comparison of these results with those before given, I have obtained the more definite views of the phenomena which are given in the following section.

Section III.

Theoretical Considerations relating to the Phenomena described in this and the preceding communications. Read November 20, 1840.

54. The experiments given in the last No. of my contributions were merely arranged under different heads, and only such inferences drawn from them as could be immediately deduced without reference to a general explanation. The addition, however, which I have since made to the number of facts, affords the means of a wider generalization; and after an attentive consideration of all the results given in this and the preceding papers, I have come to the conclusion that they can all be referred to the simple laws of the induction at the beginning and the ending of a galvanic current.

55. In the course of these investigations the limited hypotheses which I have adopted have been continually modified by the development of new facts, and therefore my present views, with the farther extension of the subject, may also require important corrections. But I am induced to believe, from its exact accordance with all the facts, so far as they have been compared, that if the explanation I now venture to give, be not absolutely true, it is so, at least, in approximation, and will therefore be of some

importance in the way of suggesting new forms of experiment, or as a first step towards a more perfect generalization.

56. To render the laws of induction at the beginning and the ending of a galvanic current more readily applicable to the explanation of the phenomena, they may be stated as follows:—1. During the time a galvanic current is increasing in quantity in a conductor, it induces, or tends to induce, a current in an adjoining parallel conductor, in an opposite direction to itself. 2. During the continuance of the primary current in full quantity, no inductive action is exerted. 3. But when the same current begins to decline in quantity, and during the whole time of its diminishing, an induced current is produced in an opposite direction to the induced current at the beginning of the primary current.

57. In addition to these laws, I must frequently refer to the fact, that *when the same quantity of electricity in a current of short duraton is passed through a galvanometer, the deflecting force on the needle is the same, whatever be the intensity of the electricity.* By intensity is here understood the ratio of a given quantity of force to the time in which it is expended;* and according to this view, the proposition stated is an evident inference from dynamic principles. But it does not rest alone on considerations of this kind, since it has been proved experimentally by Dr. Faraday, in the third series of his researches.

58. In order to form a definite conception of the several conditions of the complex phenomena which we are about to investigate, I have adopted the method often employed in physical inquiries, of representing the varying elements of action by the different parts of a curve. This artifice has been of much assistance to me in studying the subject, and without the use of it at present, I could scarcely hope to present my views in an intelligible manner to the Society.

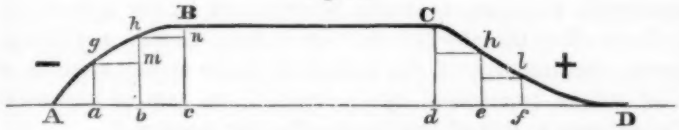
59. After making these preliminary statements, we will now proceed to consider the several phenomena; and, first, let us take the case in which the induction is most obviously produced in accordance with the laws as above stated, (56,) namely, by immersing a battery into the acid, and also by withdrawing it from

* Or more strictly speaking, the ratio of two quantities of the same species representing the force and the time.

the same. During the time of the descent of the battery into the liquid, the conductor connected with it is constantly receiving additional quantities of current electricity, and each of these additions produces an inductive action on the adjoining secondary conductor. The amount, therefore, of induced current produced during any moment of time will be just in proportion to the corresponding increase in the current of the battery during the same moment. Also, the amount of induction during any moment while the current of battery is diminishing in quantity will be in proportion to the decrease during the same moment.

60. The several conditions of this experiment may be represented by the different parts of the curve, A, B, C, D, Fig. 17, in which the distances, A*a*, A*b*, A*c*, represent the times during which the battery is descending to different depths into the acid,

Fig. 17.



and the corresponding ordinates, *ag*, *bh*, *c*B, represent the amount of current electricity in the battery conductor corresponding to these times. The differences of the ordinates, namely, *ag*, *mh*, *n*B, express the increase in the quantity of the battery current during the corresponding moments of time represented by A*a*, *ab*, *bc*; and since the inductive actions (59) are just in proportion to these *increases*, the same differences will also represent the amount of induced action exerted on the secondary conductor during the same moments of time.

61. When the battery is fully immersed in the acid, or when the current in the conductor has reached its state of maximum quantity, and during the time of its remaining constant, no induction is exerted; and this condition is expressed by the constant ordinates of the part of the curve BC, parallel to the axis. Also, the inductive action produced by each diminution of the battery current, while the apparatus is in the progress of being drawn from the acid, will be represented by the differences of the ordinates at the other end, CD, of the curve.

62. The sum of the several increments of the battery current up to its full development, will be expressed by the ordinate *c*B,

and this will, therefore, also represent the whole amount of inductive action exerted in one direction at the beginning of the primary current; and for the same reason, the equal ordinate, C*d*, will represent the whole induction in the other direction at the ending of the same current. Also, the whole time of continuance of the inductive action at the beginning and ending will be represented by A*c* and *d*D.

63. If we suppose the battery to be plunged into the acid to the same depth, but more rapidly than before, then the time represented by A*c* will be diminished, while the whole amount of inductive force expended remains the same; hence, since the same quantity of force is exerted in a less time, a greater intensity of action will be produced, (57,) and consequently a current of more intensity, but of less duration, will be generated in the secondary conductor. The relative intensity of the induced currents will, therefore, evidently be expressed by the ratio of the ordinate *c*B to the abscissa A*c*. Or in more general and definite terms, the intensity of the inductive action at any moment of time will be represented by the ratio of the rate of increase of the ordinate to that of the abscissa for that moment.*

64. It is evident from the last paragraph, that the greater or less intensity of the inductive action will be immediately presented to the eye, by the greater or less obliquity of the several parts of the curve to the axis. Thus, if the battery be suddenly plunged into the acid for a short distance, and then gradually immersed through the remainder of the depth, the varying action will be exhibited at once by the form of AB, the first part of the curve, Fig. 17. The steepness of the part A*g* will indicate an intense action for a short time A*a*, while the part *g*B denotes a more feeble induction during the time represented by *ac*. In the same way, by drawing up the battery suddenly at first, and afterwards slowly, we may produce an inductive action, such as would be represented by the parts between C and D of the ending of the curve.

65. Having thus obtained representations of the different elements of action, we are now prepared to apply these to the phe-

* According to the differential notation, the intensity will be expressed by $\frac{dy}{dx}$. In some cases the effect may be proportional to the intensity multiplied by the quantity, and this will be expressed by $\frac{dy^2}{dx}$, x and y representing, as usual, the variable abscissa and ordinate.

nomena. And, first, however varied may be the intensity of the induction expressed by the different parts of the two ends of the curve, we may immediately infer that a galvanometer, placed in the circuit of the secondary conductor, will be equally affected at the beginning and ending of the primary current; for, since the deflection of this instrument is due to the whole amount of a current, whatever may be its intensity, (57,) and since the ordinates *c*B and C*d* are equal, which represent the quantity of induction in the two directions, and, consequently, the amount of the secondary current, therefore the deflection at the beginning and ending of the battery current will, in all cases, be equal. This inference is in strict accordance with the results of experiment; for, however rapidly or slowly we may plunge the battery into the acid, and however irregular may be the rate at which it is drawn out, still, if the whole effect be produced within the time of one swing of the needle, the galvanometer is deflected to an equal degree.

66. Again, the intensity of one part of the inductive action, for example that represented by A*g*, may be supposed to be so great as to produce a secondary current capable of penetrating the body, and of thus producing a shock* while the other parts of the action, represented by *g*B and CD, are so feeble as to affect the galvanometer only. We would then have a result the same as one of those given in the last section, (42,) and which was supposed to be produced by two kinds of induction; for if the shock were referred to as the test of the existence of an induced current, one would be found at the beginning only of the battery current, while, if the galvanometer were consulted, we would perceive the effects of a current as powerful at the ending as at the beginning.

67. The results mentioned in the last paragraph cannot be obtained by plunging a battery into the acid; the formation of the current in this way is not sufficiently rapid to produce a shock. The example was given to illustrate the manner in which the same effect is supposed to be produced, in the case of the more sudden formation of a current, by plunging one end of the conductor into a cup of mercury permanently attached to a battery already

* The shock depends more on the intensity than on the quantity. See paragraph 13.

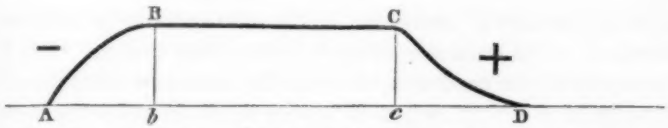
in the acid, and in full operation. The current, in this case, rapid as may be its development, cannot be supposed to assume *per saltum* its maximum state of quantity; on the contrary, from the general law of continuity we would infer, that it passes through all the intermediate states of quantity, from that of no current, if the expression may be allowed, to one of full development; there are, however, considerations of an experimental nature which would lead us to the same conclusion, (18,) (90,) and also to the farther inference that the *decline* of the current is not instantaneous. According to this view, therefore, the inductive actions at the beginning and the ending of a primary current, of which the formation and interruption are effected by means of the contact with a cup of mercury, may also be represented by the several parts of the curve, Fig. 17.

68. We have now to consider how the rate of increase or diminution of the current, in the case in question, can be altered by a change in the different parts of the apparatus; and, first, let us take the example of a single battery and a short conductor, making only one or two turns around the helix; with this arrangement, a feeble shock, as we have seen, (11,) will be felt at the making, and also at the breaking of the circuit. In this case it would seem that almost the only impediment to the most rapid development of the current would be the resistance to conduction of the metal; and this we might suppose would be more rapidly overcome by increasing the tension of the electricity; and, accordingly, we find that if the number of elements of the battery be increased, the shock at making the circuit will also be increased, while that at breaking the circuit will remain nearly the same. To explain, however, this effect more minutely, we must call to mind the fact before referred to, (17,) that when the poles of a compound battery are not connected, the apparatus acquires an accumulation of electricity, which is discharged at the first moment of contact, and which, in this case, would more rapidly develope the full current, and hence produce the more intense action on the helix at making the circuit.

69. The shock, and also the deflection of the needle, at breaking the circuit with a compound battery and a short coil, (9,) appear nearly the same with a battery of a single element, because the accumulation just mentioned, in the compound battery, is discharged almost instantly, and, according to the theory (71) of

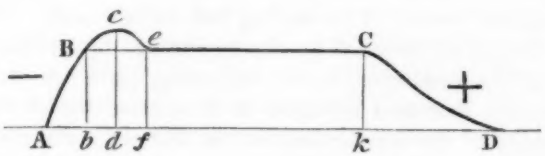
the galvanic current, leaves the constant current in the conductor nearly in the same state of quantity as that which would be produced by a battery of a single element; and hence the conditions of the ending of the current are the same in both cases. Indeed, in reference to the ending induction, it may be assumed as a fact which is in accordance with all the experiments, (9, 13, 73, 74, 75, 76, &c.,) as well as with theoretical considerations,* *that when the circuit is broken by a cup of mercury, the rate of the diminution of the current, within certain limits, remains the same, however the intensity of the electricity or the length of the conductor may be varied.*

Fig. 18.



70. The several conditions of the foregoing examples are exhibited by the parts of the curves, Figs. 18 and 19. The gradual development of the current in the short conductor, with a single battery, and the gradual decline of the same, are represented by the gentle rise of AB and fall of CD, Fig. 18; while, in the next Fig. (19,) the sudden rise of AB indicates the intensity which produces the increased shock, after the number of elements of the battery has been increased. The accumulation of the electricity, which almost instantly subsides, is represented by the part B*ce*, Fig. 19, and from this we see at once, that although the

Fig. 19.



shock is increased by using the compound battery, yet the needle of the galvanometer will be deflected only to the same number of degrees, since the parts Bc and *ce* give inductive actions in

* See the theory of Ohm.

contrary directions, and both within the time of a single swing of the needle, and, consequently, will neutralize each other. The resulting deflecting force will, therefore, be represented by *ef*, which is equal to C*k*, or to *b*B, in Fig. 18.

The intensity of the shock at the breaking is represented as being the same in the two figures, by the similarity of the rate of descent of the part CD of the curve in each.

71. We have said (69) that the quantity of current electricity in a short conductor and a compound battery, after the first discharge, is nearly the same as with a single battery. The exact quantity, according to the theory of Ohm, in a unit of length of the conductor is given by the formula $\frac{nA}{rn+R}$. In this, n represents the number of elements; A, the electromotive force of one element; r, the resistance to conduction of one element; and R, the length of the conductor, or rather its resistance to conduction in terms of r. Now, when R is very small, in reference to rn, as is the case with a very short metallic conductor, it may be neglected, and then the expression becomes $\frac{nA}{rn}$ or $\frac{A}{r}$; and since this expresses the quantity of current electricity in a unit of the length of the circuit, with either a single or a compound battery, therefore, with a short conductor, the quantity of current electricity in the two cases is nearly the same.

72. Let us next return to the experiment with a battery of a single element, (68,) and instead of increasing the intensity of the apparatus, as in the last example, let the length of the conductor be increased; then the intensity of the shock at the beginning of the current, as we have seen, (14,) will be diminished, while that of the one at the ending will be increased. That the shock should be lessened at the beginning, by increasing the length of the conductor, is not surprising, since, as we might suppose, the increased resistance to conduction would diminish the rapidity of the development of the current. But the secondary current, which is produced in the conductor of the primary current itself, as we have seen, (19,) is the principal cause which lessens the intensity of the shock; and the effect of this, as will be shown hereafter, may also be inferred from the principles we have adopted.

73. The explanation of the increased shock at the moment of breaking the circuit with the long conductor, rests on the assumption before mentioned, (69,) that the velocity of the diminution of a current is nearly the same in the case of a long conductor as in that of a short one. But, to understand the application of this principle more minutely, we must refer to the change which takes place in the quantity of the current in the conductor by varying its length; and this will be given by another application of the formula before stated, (71.) This, in the case of a single battery, in which n equals unity, becomes $\frac{A}{r+R}$; and since this, as will be recollected, represents the quantity of current electricity in a unit of length of the conductor, we readily infer from it, that by increasing the length of the conductor, or the value of R, the quantity of current in a unit of the length is lessened. And if the resistance of a unit of the length of the conductor were very great in comparison with that of r, (the resistance of one element of the battery,) then the formula would become $\frac{A}{R}$, or the quantity in a single unit of the conductor would be inversely as its entire length, and hence the amount of current electricity in the whole conductor would be a constant quantity, whatever might be its length. This, however, can never be the case in any of our experiments, since in no instance is the resistance of R very great in reference to r, and therefore, according to the formula, (73,) the whole quantity of current electricity in a long conductor is always somewhat greater than in a short one.

74. Let us, however, in order to simplify the conditions of the induction at the ending of a current, suppose that the quantity in a unit of the conductor is inversely as its whole length, or, in other words, that the quantity of current electricity is the same in a long conductor as in a short one; and let us also suppose, for an example, that the length of the spiral conductor, Fig. 3, was increased from one spire to twenty spires; then, if the velocity of the diminution of the section of the current is the same (69) in the long conductor as in the short one, the shock which would be received by submitting the helix to the action of one spire of the long coil would be nearly of the same intensity as that from one spire of the short conductor; the quantity of induction, however, as shown by the galvanometer, should be nearly twenty

times less; and these inferences I have found in accordance with the results of experiments, (75.) If, however, instead of placing the helix on one spire of the long conductor, it be submitted at once to the influence of all the twenty spires, then the intensity of the shock should be twenty times greater, since twenty times the quantity of current electricity collapses, if we may be allowed the expression, in the same time, and exerts at once all its influence on the helix. If, in addition to this, we add the consideration that the whole quantity of current electricity in a long conductor is greater than that in a short one, (73,) we shall have a further reason for the inc ease of the terminal shock, when we increase the length of the battery conductor.

75. The inference given in the last paragraph, relative to the change in the quantity of the induction, but not in the intensity of the shock from a single spire, by increasing the whole length of the conductor, is shown to be true by repeating the experiment described in paragraph 13. In this, as we have seen, the intensity of the shock remained the same, although the length of the circuit was increased by the addition of coil No. 2. When, however, the galvanometer was employed in the same arrangement, the whole quantity of induction, as indicated by the deflection of the needle, was diminished almost in proportion to the increased length of the circuit. I was led to make this addition to the experiment (13) by my present views.

76. The explanation given in paragraph 74, also includes that of the peculiar action of a long conductor, either coiled or extended, in giving shocks and sparks from a battery of a single element, discovered by myself in 1831; (see Contrib. No. II.) The induction, in this case, takes place in the conductor of the primary current itself, and the secondary current which is produced is generated by the joint action of each unit of the length of the primary current. Let us suppose, for illustration, that the conductor was at first one foot long, and afterwards increased to twenty feet. In the first case, because the short conductor would transmit a greater quantity of electricity, the secondary current produced by it would be one of considerable quantity, or power to deflect a galvanometer; but it would be of feeble intensity, for although the primary current would collapse with its usual velocity, (69,) yet, acting on only a foot of conducting matter, the effect (74) would be feeble. In the second case, each foot

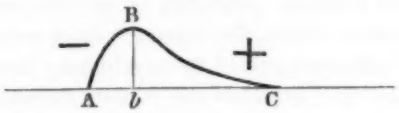
of the twenty feet of the primary current would severally produce an inductive action of the same intensity as that of the short conductor, the velocity of collapsion being the same; and as they are all at once exerted on the same conductor, a secondary current would result of twenty times the intensity of the current in the former case.

77. To render this explanation more explicit, it may be proper to mention that a current produced by an induction on one part of a long conductor of uniform diameter, must exist, of the same intensity, in every other part of the conductor; hence, the action of the several units of length of the primary current must enforce each other, and produce the same effect on its own conductor that the same current would if it were in a coil, and acting on a helix. I need scarcely add, that in this case, as in that given in paragraph 74, the whole amount of induction is greater with the long conductor than with the short one, because the quantity of current electricity is greater in the former than in the latter.

78. We may next consider the character of the secondary current, in reference to its action in producing a tertiary current in a third conductor. The secondary current consists, as we may suppose, in the disturbance, for an instant, of the natural electricity of the metal, which, subsiding, leaves the conductor again in its natural state; and whether it is produced by the beginning or ending of a primary current, its nature, as we have seen, (22,) is the same. Although the time of continuance of the secondary current is very short, still we must suppose it to have some duration, and that it increases, by degrees, to a state of maximum development, and then diminishes to the normal condition of the metal of the conductor; the velocity of its development, like that of the primary current, will depend on the intensity of the action by which it is generated, and also, perhaps, in some degree, on the resistance of the conductor; while, agreeably to the hypothesis we have assumed, (69,) the velocity of its diminution is nearly a constant quantity, and is not affected by changes in these conditions; hence, if we suppose the induction which produces the secondary current to be sufficiently intense, the velocity of its development will exceed that of its diminution, as in the example of the primary current from the intense source of the compound battery of many elements. Now this is the case with the inductions which produce currents of the different or-

ders, capable of giving shocks or of magnetizing steel needles; the secondary currents from these are always of considerable intensity, and hence their rate of development must be greater than that of their diminution, and, consequently, they may be represented by a curve of the form exhibited in Fig. 20, in which there is no constant part, and in which the steepness of AB is

Fig. 20.



greater than that of BC. There are, however, other considerations, which will be noticed hereafter, (89,) which may affect the form of the part BC of the curve, Fig. 20, rendering it still more gradual in its descent, or, in other words, which tend to diminish the intensity of the ending induction of the secondary current.

79. It will be seen at once, by an inspection of the curve, that the effect produced, in a third conductor, and which we have called a tertiary current, is not of the same nature as that of a secondary current. Instead of being a single development in one direction, it consists of two instantaneous currents, one produced by the induction of AB, and the other, by that of BC, in opposite directions, of equal quantities, but of different intensities. The whole quantity of induction in the two directions, will each be represented by the ordinate B*b*, and hence they will nearly neutralize each other, in reference to their action on the galvanometer, in the circuit of the third conductor. I say, they will *nearly* neutralize each other, because, although they are equal in quantity, they do not both act in absolutely the same moment of time. The needle will, therefore, be slightly affected; it will be impelled in one direction, say to the right, by the induction of AB, but, before it can get fairly under way, it will be arrested, and turned in the other direction, by the action of BC. This inference is in strict accordance with observation; the needle, as we have seen, (24,) starts from a state of rest, with a velocity which, apparently, would send it through a large arc, but before it has reached, perhaps, more than half a degree, it suddenly stops, and turns in the other direction. As the needle is first affected by the action of AB, it indicates a current in the adverse direction to the secondary current.

80. Although the two inductions in the tertiary conductor nearly neutralize each other, in reference to the indications of the galvanometer, yet this is far from being the case with regard to the shocks, and the magnetization of steel needles. These effects may be considered as the results alone of the action of AB; the induction of BC being too feeble in intensity to produce a tertiary current of sufficient power to penetrate the body, or overcome the coercive power of the hardened steel. Hence, in reference to the shock, and magnetization of the steel needle, we may entirely neglect the action of BC, and consider the tertiary excitement as a single current, produced by the action of AB; and, because this is the beginning induction, (56,) the tertiary current must be in an opposite direction to the secondary. For a similar reason, a current of the third order should produce in effect a single current of the fourth order, in a direction opposite to that of the current which produced it, and so on: we have here, therefore, a simple explanation of the extraordinary phenomenon of the alternation of the directions of the currents, of the different orders, as given in this (25) and the preceding paper.

81. The operation of the interposed plate, (32, 47, 48, &c.,) in neutralizing the shock, and not affecting the galvanometer, can also be readily referred to the same principles. It is certain, that an induced current is produced in the plate, (III, 64,) and that this must react on the secondary, in the helix; but it should not alter the total amount of this current, since, for example, at the ending induction, the same quantity of current is added to the helix, while the current in the plate is decreasing, as is subtracted while the same current is increasing. To make this more clear, let the inductive actions of the interposed current be represented by the parts of the curve, Fig. 20. The induction represented by AB will react on the current in the helix, and diminish its quantity, by an amount represented by the ordinate bB; but the induction represented by BC, will act in the next moment, on the same current, and increase its quantity by an equal amount, as represented by the same ordinate Bb; and since both actions take place within a small part of the time of a single swing of the needle, the whole deflection will not be altered, and consequently, as far as the galvanometer is concerned, the interposition of the plate will have no perceptible effect.

82. But the effect of the plate on the shock, and on the magnetization of tempered steel, should be very different; for although the quantity of induction in the helix may not be changed, yet its intensity may be so reduced, by the adverse action of the interposed current, as to fall below that degree which enables it to penetrate the body, or overcome the coercive force of the steel. To understand how this may be, let us again refer, for example, to the induction which takes place at the ending of a battery current: this will produce, in both the helix and the plate, a momentary current, in the direction of the primary current, which we have called *plus;* the current in the plate will react on the helix, and tend to produce in it two inductions, which, as before, may be represented by AB, and BC, of the curve, Fig. 20; the first of these, AB, will be an intense action, (78,) in the *minus* direction, and will, therefore, tend to neutralize the intense action of the primary current on the helix; the second, (BC,) will add to the helix an equal quantity of induced current, but of a much more feeble intensity, and hence the resulting current in the helix will not be able to penetrate the body; no shock will be perceived, or at least a very slight one, and the phenomena of screening will be exhibited.

83. When the plate of metal is placed between the conductors of the second and third orders, or between those of the third and fourth, the action is somewhat different, although the general principle is the same. Let us suppose the plate interposed between the second and third conductors; then the helix, or third conductor, will be acted on by four inductions, two from the secondary current, and two from the current in the plate. The direction and character of these will be as follows, on the supposition that the direction of the secondary current is itself *plus:*

The beginning secondary .	intense and . .	*minus.*
The ending secondary . .	feeble and . .	*plus.*
The beginning interposed .	intense and . .	*plus.*
The ending interposed . .	feeble and . .	*minus.*

Now if the action, on the third conductor, of the first and third of the above inductions be equal in intensity and quantity, they will neutralize each other; and the same will also take place with the action of the second and fourth, if they be equal, and hence, in this case, neither shock nor motion of the needle of the

galvanometer would be produced. If these inductions are not precisely equal, then, only a partial neutralization will take place, and the shock will only be diminished in power; and, also, perhaps, the needle will be very slightly affected.

84. If, in the foregoing exposition, we throw out of consideration the actions of the feeble currents which cannot pass the body, and, consequently, are not concerned in producing the shock, then the same explanation will still apply which was given in the last paper, (III, 94,) namely, in the above example, the helix is acted on by the minus influence of the secondary, and the plus influence of the interposed current.

85. We are now prepared to consider the effect on the helix (Fig. 3) of the induced currents produced in the conductor of the primary current itself. These are true secondary currents, and are almost precisely the same in their action as those in the interposed plate. Let us first examine the induced current at the beginning of the primary, in the case of a long coil and a battery of a single element; its action on the helix may be represented by the parts of the curve, Fig. 20. The first part, AB, will produce an intense induction opposite to that of the primary current; and hence the action of the two will tend to neutralize each other, and no shock, or a very feeble one, will be produced. The ending action of the same induced current, which is represented by BD, restores to the helix the same quantity of current electricity (but in a feeble state) which was neutralized by AB, and hence the needle of the galvanometer will be as much affected as if this current did not exist. These inferences perfectly agree with the experiment given in paragraph 19. In this, when the ends of the interposed coil were joined so as to neutralize the induced current in the long conductor, the shock at the beginning of the primary current was nearly as powerful as with a short conductor, while the amount of deflection of the galvanometer was unaffected by joining the ends of the same coil.

86. At first sight it might appear that any change in the apparatus which might tend to increase the induction of the primary current (16) would also tend to increase, in the same degree, the adverse secondary in the same conductor; and that hence the neutralization mentioned in the last paragraph would take place in all cases; but we must recollect that if a more full current be suddenly formed in a conductor of a given thickness, the ad-

verse current will not have, as it were, as much space for its development, and therefore, will have less power in neutralizing the induction of the primary than before. But there is another, and, perhaps, a better reason, in the consideration that in the case of the increase of the number of elements of the battery, although the rapidity of the development of the primary current is greater, yet the increased resistance which the secondary meets with, in its motion against the action of the several elements, will tend to diminish its effect. Also, by diminishing the length of the primary current, we must diminish (76) the intensity of the secondary, so that it will meet with more resistance in passing the acid of the single battery, and thus its effects be diminished.

87. The action of the secondary current, in the long coil at the *ending* of the primary current, should, also, at first sight, produce the same screening influence as the current in the interposed plate; but, on reflection, it will be perceived that its action in this respect must be much more feeble than that of the similar current at the beginning; the latter is produced at the moment of making contact, and hence it is propagated in a continuous circuit of conducting matter, while the other takes place at the *rupture* of the circuit, and must therefore be rendered comparatively feeble by being obliged to pass through a small portion of heated air; very little effect is therefore produced on the helix by this induction, (19.) The fact that this current is capable of giving intense shocks, when the ends of a long wire, which is transmitting a primary current, are grasped at the time of breaking the circuit, is readily explained, since, in this case, the body forms, with the conductor, a closed circuit, which permits the comparatively free circulation of the induced current.

88. It will be seen that I have given a peculiar form to the beginning and ending of the curves, Figs. 17, 18, &c. These are intended to represent the variations which may be supposed to take place in the rate of increase and decrease of the quantity of the current, even in the case where the contact is made and broken with mercury. We may suppose, from the existence of analogous phenomena in magnetism, heat, &c., that the development of the current would be more rapid at first than when it approximates what may be called the state of current saturation, or when the current has reached more nearly the limit of capacity of conduction of the metal. Also, the decline of the current may

be supposed to be more rapid at the first moment, than after it has lost somewhat of its intensity, or sunk more nearly to its normal state. These variations are indicated by the rapid rise of the curve, Fig. 17, from A to *g*, and the more gradual increase of the ordinates from *h* to B; and by the rapid diminution of the ordinates between C and *l*, and the gradual decrease of those towards the end of the curve.

89. These more minute considerations, relative to the form of the curve, will enable us to conceive, how the time of the ending of the secondary current, as we have suggested, (78,) may be prolonged beyond that of the natural subsidence of the disturbance of the electricity of the conductor on which this current depends. If the development of the primary current is produced by equal increments in equal times, as would be the case in plunging the battery (59) into the acid with a uniform velocity; then the part AB of the curve Fig. 17, would be a straight line, and the resulting secondary current, after the first instant, would be one of constant quantity during nearly the whole time represented by A*c*; but if the rate of the development of the primary current be supposed to vary in accordance with the views we have given in the last paragraph, then the quantity of the secondary current will begin to decline before the termination of the induction, or as soon as the increments of the primary begin to diminish; and hence the whole time of the subsidence of the secondary will be prolonged, or the length of *b*C, Fig. 20, will be increased, the descent of BC be more gradual, and the intensity of the ending induction of the secondary current be diminished: (see last part of paragraph 78.)

90. Besides the considerations we have mentioned, (88,) there are others of a more obvious character, which would also appear to affect the form of particular parts of the curve. And first we might perhaps make a slight correction in the drawing of Figs. 17, 18, &c., at the point A, in consideration of the fact that the very first contact of the end of the conductor with the surface of the mercury is formed by a point of the metal, and hence the increment of development should be a little less rapid at the first moment than after the contact has become larger; or in other words, the curve should perhaps start a little less abruptly from the axis at the point A. Also Dr. Page has stated* that he finds

* Vide this Journal, Vol. xxxiv, p. 166.

the shock increased by spreading a stratum of oil over the surface of the mercury; in this case it is probable that the termination of the current is more sudden, on account of the prevention of the combustion of the metal by means of the oil, and the fact that the end of the conductor is drawn up into a non-conducting medium.

91. The time of the subsidence of the current, when the circuit is broken by means of a surface of mercury, is very small, and probably does not exceed the ten thousandth part of a second, but even this is an appreciable duration, since I find that the spark at the ending presents the appearance of a band of light of considerable length, when viewed in a mirror revolving at the rate of six hundred times in a second; and I think the variations in the time of ending of the current under different conditions may be detected by means of this instrument.

92. Before concluding this communication, I should state that I have made a number of attempts to verify the suggestion given in my last paper, (III, 127,) that an inverse induction is produced by a galvanic current by a change in the distance of the conductors, but without success. These attempts were made before I had adopted the views given in this section, and since then I have found (80) a more simple explanation of the alternation of the currents.

93. In this number of my contributions, the phenomena exhibited by the galvanic apparatus have alone been discussed. I have, however, made a series of experiments on the induction from ordinary electricity, and the reaction of soft iron on currents, and I think that the results of these can also be referred to the simple principles adopted in this paper; but they require further examination before being submitted to the public.

ART. XV.—*A Brief, Preliminary Account of the Hessian Fly and its Parasites;* by EDWARD C. HERRICK, Mem. Yale Nat. Hist. Soc.

[Communicated to the Yale Nat. Hist. Society, April 28, 1841.]

FOR several years past I have spent some time in the study of the habits of the Hessian fly, and of the various insects by which it is attacked. During a part of the period I enjoyed the important coöperation of my valued friend, Mr. James D. Dana, now absent from the country, as one of the scientific corps of the United States South Sea Exploring Expedition. It was, and still continues to be, my intention, to offer an extended paper on this subject. The investigation is not yet in every particular so complete as could be wished, but several circumstances seem to render it advisable to give at this time, a brief abstract of some portion of the results. The civil history of the insect, as well as the scientific descriptions, with many other details, are reserved for the final paper.

The Hessian fly, which has so long been conspicuous for its depredations on the wheat crops of this country, is a two-winged insect of the genus *Lasioptera* or *Cecidomyia*, (*Meig.* and *Latr.*) and was first scientifically described by the late Mr. Thomas Say, (*Jour. Acad. Nat. Sci. Phil.* 1817, i, 45,) who gave it the specific name of *Destructor*. The popular name was first used by Col. George Morgan, of Prospect, N. J., on the supposition that the insect was introduced into this country among the straw brought by the Hessian troops who came here in the service of Great Britain, during the war of the Revolution. This supposition has been rejected by most entomologists, chiefly perhaps, because an extensive and apparently thorough inquiry made in various parts of Europe a few years after, resulted in the uncontradicted conclusion that the insect was wholly unknown in that quarter of the world. I am not prepared to assert that this insect was introduced in the manner above supposed, but it may be shown that it is highly probable that it was unknown here before that time; that it now exists in Europe, and has probably been there for centuries.

In the *Eléments d'Agriculture, par Duhamel du Monceau, Paris*, 1771, 2 *tomes*, 12mo., is a statement from M. de Châteauvieux, that the insect was first observed in Europe in 1778, in the neighborhood of Turin, in Piedmont. It is also mentioned in the *Transactions of the Society of Arts, London*, vol. iii, 1785, as having appeared in Flanders in 1779. In the *Transactions of the Society of Arts, Edinburgh*, vol. i, 1788, it is described as having appeared in Scotland in 1782. In the *Transactions of the Society of Arts, London*, vol. iii, 1785, it is described as having appeared in Flanders in 1779. In the *Transactions of the Society of Arts, Edinburgh*, vol. i, 1788, it is described as having appeared in Scotland in 1782.

vieux, of which the following is a translation:—"Our wheat [in the neighborhood of Geneva] has sustained the present month of May, 1755, an injury, from which the grain cultivated by the new husbandry has not been exempt. We found upon it a number of small white worms, which eventually turned to a chestnut color: they fix themselves within the leaves, and gnaw the stalks. They are commonly found between the first joint and the root: the stalks on which they fasten, grow no more; they become yellow and dry up. We suffered the same injury in 1732, when these insects appeared in the middle of May, and did such damage that the crops were almost annihilated." i, 299. This passage was quoted by Col. Morgan, (*Carey's Amer. Mus.* 1787, i, 530,) in the belief that the insect described in it was the Hessian fly. The description is too imperfect to authorize a positive assertion, but there seems to be little doubt that his opinion is correct.

In 1833, Mr. Dana sailed for the Mediterranean in the U. S. ship Delaware. An opportunity was thus afforded him to make personal exploration for the Hessian fly among the wheat fields of the old world; a work for which he was well prepared by his thorough acquaintance with this insect in its various stages. His examinations were rewarded with the most gratifying success, for they proved that *the Hessian fly is an inhabitant of Europe.* On the 13th of March, 1834, and subsequently, he collected several larvæ and pupæ, from wheat plants growing in a field on the island of Minorca. From these pupæ, were evolved on the 16th of March, 1834, two individuals of an insect which his recollections, (aided by a drawing of the Hessian fly with which he was provided,) enabled him to pronounce to be the *Cecidomyia Destructor.* More of the perfect insects were evolved in the course of the month, one of which deposited eggs like those of the Hessian fly. In letters dated Mahon, April 8 and 21, 1834, Mr. D. sent me five of the insects, and several of the pupæ. They arrived in safety, and after a careful examination, I saw no good reason to doubt the identity of this insect with the Hessian fly. The Mahonese asserted that the insect had been there from time immemorial, and often did great damage both there and in Spain. On the 28th of April, 1834, Mr. D. collected from a wheat field just without the walls of the city of *Toulon* in France, several pupæ and one larva like those before obtained. On

the 4th of June, 1834, he obtained similar pupæ from a wheat field near *Naples*.—About the period of Mr. Dana's investigations in the south of Europe, attention was turned to the injury caused by certain larvæ among the wheat in Hungary. It appears now to be commonly believed, that their parent insect is either our Hessian fly, or an animal very closely allied to it.

I have searched in vain for any traces of the Hessian fly in this country before the Revolution. The Rev. Jared Eliot, in his "*Essays upon Field Husbandry in New England*," Boston, 1760, treats of the culture of wheat, but makes no allusion to any insect having habits like those of the Hessian fly; neither does Kalm, the naturalist, who travelled in this country about 1750. I am therefore inclined to consider the common opinion of the origin of the insect quite as probable as any other which has been advanced.

In this part of our country, wheat is usually sown about the first of September. Soon after the plants are up, the Hessian fly begins to lay her eggs upon them, and continues her operations for several weeks. She deposits her eggs on the upper surface of the leaf (i. e. the *ligula*, or strap-shaped portion of the leaf) of the plant. The number on a single leaf is often twenty or thirty, and sometimes much greater. In these cases many of the larvæ must perish. The egg is about a fiftieth of an inch long, and four hundredths of an inch in diameter, cylindrical, translucent, and of a pale red color. In about four days the egg hatches; the young larva creeps down the leaf, enters the sheath, and with the head downwards, fastens upon the tender culm or stalk, generally just above some joint. The larva appears to feed solely on the sap of the plant; it does not gnaw the stalk, and never enters it, but is gradually imbedded in it as the plant matures. Having taken its post, the larva is stationary; it gradually loses its reddish color, becomes translucent, and clouded with white spots, and when near maturity, the central part within is of a greenish hue. In about five or six weeks, (or longer if the season is cold,) the larva begins to assume a brownish tinge, and soon is of a bright chestnut color, when the insect may be said to have reached the state of *pupa*. It has then some resemblance to a flax-seed. The outer skin of the larva becomes the puparium of the pupa. The wheat plant is injured by the loss of sap, but principally by the pressure of the larvæ and pupæ upon the culm. A single

larva will do little harm, (and may even be useful by stimulating the plant to throw out side shoots,) but five or six of them are sufficient seriously to check the growth of the plant, or perhaps to destroy it entirely.*

During the winter the insect is in the pupa state, near the root of the wheat plant, and usually a little below the surface of the earth. In April and May we again find the Hessian fly laying eggs on the young wheat, both that which was sown in the autumn previous, and the spring wheat, which is of course recently up. The larvæ from these eggs become pupæ about the middle of June.

There is no difficulty in tracing the insect as far as the state of pupa, and to this point its history is satisfactorily ascertained. Regarding the periods of the evolution of the perfect insect, there is, however, some obscurity, which numerous observations have not wholly cleared up. The difficulty results in part from the fact that in this region, a very large proportion, probably *more than nine tenths*, of every generation of the Hessian fly, is destroyed by parasites. A great part of the pupæ which may be collected will evolve some parasitic insect, instead of the Hessian fly. It is certain that sometimes, the pupæ, which became so in June, evolve the perfect insect in October following, and that other pupæ of the same date will not evolve the perfect insect until October of the year succeeding. The following seems to me the *probable* history of the matter. The pupæ, which became such in the autumn, evolve the perfect insect, partly during the next spring, and partly in the summer and autumn following.

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* It has been repeatedly asserted that the Hessian fly lays her eggs on the ripening grain. This error has doubtless arisen from mistaking for the Hessian fly, other insects, which in various parts of our country attack the wheat. In the *Trans. of the Amer. Phil. Soc.*, 1771, i, 205, Col. Landon Carter has given some account of a pale brownish moth, called by him the *fly weevil that destroys the wheat*, which lays its eggs on the grain. A paper on what is probably the same insect, is published by John Lorain in Mease's *Archives of Useful Knowledge*, 1812, ii, 47. The insect is supposed to be that described by Duhamel du Monceau in his "*Histoire d'un Insecte qui devore les Grains de l'Angoumois*," Paris, 1762. 12mo.—the *Œcophora cerealella*, Oliv. I have little doubt that the *Tipula* (*Cecidomyia*) *Tritici*, Kirby, (*Trans. Linn. Soc.* iii, iv, v,) also inhabits this country.—In August, 1833, I received several stalks of wheat from West Chester, Penn., each containing, in the centre, a small larva of a wax-yellow color; but I failed to obtain the perfect insect. It is hardly necessary to say that this was not the larva of the Hessian fly. A critical investigation of all these insects is very much to be desired.

The pupæ, which became such in and about June, evolve the perfect insect, partly during the next autumn, and partly during the year succeeding.

Parasites.—There are in this region, *four* principal parasites of the Hessian fly, one of which attacks the eggs, and the other three the pupæ. They are all minute *Hymenoptera.*

1. The egg-parasite, is a species of *Platygaster*, Latr., and may prove to be identical with some one of the hundred species of this genus which are described. (Entom. Mag. Lond. iii, 217. Cont. Macl. Lyc. i, 81.) The insect is abundant in the autumn. I first saw it Sept. 23, 1833, in the act of depositing its eggs in the eggs of the Hessian fly. From subsequent observations it appears that four or five eggs are laid in a single egg of the Hessian fly. The latter egg hatches, and the animal advances to the pupa state as usual, but from the puparium no Hessian fly ever comes forth. This parasite forms within the puparium, a silky cocoon of a brownish color.

2. This is the chief parasite of the pupa. It is described by Mr. Say, (Jour. Acad. Nat. Sci. Phil. i, 47,) as the *Ceraphron Destructor.* It appears to me not to belong to the genus *Ceraphron*, (Latr.) but to fall within the genus *Eurytoma*, of Illiger. It pierces the sheath of the stalk, (making a hole too small to be detected by a powerful microscope,) and deposits an egg in the pupa within. This is done chiefly in June. The perfect insect is evolved in the summer and autumn succeeding, eating its way through the puparium and the sheath of the leaf. An insect (of which I have seen females only) very similar to the Eurytoma Destructor, but with mere rudiments of wings, is sometimes evolved from the pupæ of the Hessian fly. I am in doubt whether it should be considered a distinct species or only a variety. The winged individuals never throw off their wings.

3. The next parasite of the pupa, is an insect of the tribe Chalcidiæ, (*Latr.* in *Cuv. Règne An.*) whose genus I have not determined. Its habits are like those of No. 2, but it is evolved later. Apterous females of this species are also found.

4. Another parasite of the pupa is an insect of the tribe Oxyuri, (*Latr.* in *Cuv.*) whose genus I have not determined. In habits it agrees with Nos. 2 and 3, but it is evolved still later in the year. All of these parasites are likewise evolved in the spring, from Hessian fly pupæ of the summer previous.

A few suggestions may be made respecting the best modes of preventing the ravages of the Hessian fly. They have all been published before, by others, but they are of such a nature that there is little probability that any of them will ever exterminate the insect. The stouter varieties of wheat ought always be chosen, and the land should be kept in good condition. If fall wheat is sown late, some of the eggs will be avoided, but risk of winter-killing the plants will be incurred. If cattle are permitted to graze the wheat fields during the fall, they will devour many of the eggs. A large number of the pupæ may be destroyed by burning the wheat-stubble immediately after harvest, and then ploughing and harrowing the land. This method will undoubtedly do much good. As the Hessian fly also lays its eggs, to some extent, on rye and barley, these crops should be treated in a similar manner.

New Haven, Conn.

Art. XV.—*Proceedings of Learned Societies.*

I. *Association of American Geologists.*

The second annual meeting of this Association was held during the second week in April, at the rooms of the Academy of Natural Sciences in Philadelphia. The following is an abstract of the proceedings.

Monday, April 5th, 1841, 4 o'clock, P. M.—The Association met pursuant to the adjournment of last year. The regularly appointed presiding officers being absent, Prof. Henry D. Rogers was called to the chair. After the completion of some business arrangements, the Association adjourned until 10 o'clock, A. M. of Tuesday.

Tuesday, April 6th, 1841, 10 o'clock, A. M.—The Association met pursuant to adjournment. *Prof. Silliman* took the chair. *Dr. L. C. Beck* was appointed secretary. *Messrs. W. R. Johnson, Vanuxem, H. D. Rogers, Mather*, and *Locke*, were appointed a committee to report a plan of business for the meeting.

The subject of mineral manures having been referred at the last annual meeting, was proposed for discussion. Remarks were offered, and facts stated by *Mr. Martin H. Boyé, Drs. Charles T. Jackson, James B. Rogers, J. Locke*, and *Mr. B. Silliman, Jr.*

Mr. Boyé enquired whether the antacid powers of magnesia, and its effects on vegetation, had been noticed, as it exists in the dolomitic limestone.

Dr. Jackson inferred from his observations, that magnesia is injurious only when used in a caustic state, in the same manner as caustic lime is known to be injurious to vegetation by abstracting carbonic acid from the atmosphere, and from decomposing vegetable and animal matters. It also acts unfavorably in virtue of its hydraulic power; rendering in some cases, the soil very hard. He further stated, that when composted with peat and swamp muck it gained from these substances phosphoric acid, and thus became the means of conveying to wheat and other cereal grains the phosphate of magnesia, known to be always found in their ashes. Dr. Jackson further considered the combinations of lime with the organic acids of soils as deserving much attention. He had found subsoils to contain a larger quantity of crenates of lime than the soil, and that some streams in like manner contained a larger proportion of soluble crenates than others, and these former are most valuable for the purposes of irrigation. Dr. J. recommends the employment of a compost of lime, swamp muck or peat, and animal manure, and he attributes the beneficial effect of this in part to the evolution of ammonia consequent upon the decomposition of the organic matters.

The discussion then turned on the character of hydrated peroxide of iron on vegetation. It was thought by Dr. Jackson, that the injurious effects sometimes known to arise from it were to be attributed to free sulphuric acid contained in it from the decomposition of sulphuret of iron. A marl was cited which at first produced very luxuriant vegetation, but at a subsequent period was found to destroy the plants growing where it had been used, owing to the decomposition of sulphuret of iron contained in it, producing free sulphuric acid, which corroded the plants. *Prof. H. D. Rogers*, thought that some of the marls of *New Jersey*, contained so much sulphuret of iron as to require more alkaline matter than was to be found in them to neutralize the acid resulting from their decomposition. Still it was thought that *small quantities* of sulphuret of iron, in a marl, would by its decomposition be useful in agriculture.

The subject of potash in soils was next brought before the meeting. *Dr. Jackson* inquired if any experiments had been

made on this subject by gentlemen present. He had digested soils from Maine, New Hampshire, and Rhode Island, with boiling water, without discovering more than a trace of potash; while the method proposed by Mitscherlich of digesting the soils in free sulphuric acid, always gave decided indications of potash. He was led to infer, therefore, that the mica and other minerals containing potassa were by this method decomposed.

Mr. B. Silliman, Jr. stated, that the soil of the Nile, when treated according to the method of Mitscherlich, gave abundance of potash, but not any appreciable quantity with boiling water; he was therefore led to believe that the mica, contained abundantly in the soil, was decomposed by the sulphuric acid.

Resolved, That a committee be appointed to prepare a detailed report upon the subject of soils and mineral manures, embodying as well the fruits of their own investigations as the results arrived at by others, and that the same be presented at the next meeting.

Drs. C. T. Jackson, Robert Rogers, Mr. M. Boyè, Dr. L. C. Beck, Dr. W. Horton, Mr. B. Silliman, Jun., and *Prof. Booth*, were appointed on the above committee.

The committee appointed to prepare a plan of business, made a report, which was adopted.

Prof. Mather asked for and obtained leave to defer his report on "Drift," until the next meeting of the Association; in the mean time he was requested to make an oral communication on this subject during the present meeting.

Prof. Locke read a paper "On the Geology of some parts of the United States west of the Allegany Mountains."

In this paper the author exhibited particularly the points of agreement between the lead region of the upper Mississippi, and that of Derbyshire in England, and between the mountain limestone of Europe and the "cliff limestone" of the west. He showed that the two rocks agree in geological position, in external and chemical characters, in fossil remains, and in metallic veins; being both highly metalliferous and abounding in lead and zinc ores occupying vertical fissures. He described the upper, middle, and lower beds of the "cliff limestone" of the lead region of the west as differing somewhat in characters and in fossil remains, and suggested the inquiry whether these three beds, together with the blue fossiliferous limestone which underlies them, (the probable equivalent of the Trenton limestone,) and the alternations of the lower magnesian limestone with the saccharoid sandstone, found at Prairie du Chien, should be considered distinct formations, (as their fossil remains would to some extent

indicate,) or as different members of one formation—the mountain limestone.

In reply to some remarks by *Prof. H. D. Rogers*, *Prof. Locke* observed, that he did not undertake to be the advocate of absolute equivalency, but merely to point out the agreement and disagreement of certain formations in America with similar ones in Europe. He was of opinion, however, that certain points of equivalency must be admitted, as for example granite, the great coal formations, &c.

Prof. Mather proposed the subject of "Joints of Rocks" for discussion during this session of the Association; and *Prof. H. D. Rogers* proposed that of "Fossil and Recent Infusoria."

The Association then adjourned until 4 o'clock this afternoon.

April 6th, 1841, 4 o'clock, P. M.—The Association met pursuant to adjournment, *Prof. Locke* in the chair.

Mr. William C. Redfield exhibited specimens of fossil shells, from the tertiary marl-beds at Washington, Beaufort county, North Carolina.

Mr. R. stated that these beds, which are about sixty miles from the Atlantic, are found from fifteen to twenty feet below the adjacent surface, and two or more feet lower than the usual level of Pamlico river and sound. The fossils are in a good state of preservation, and are supposed to belong to the miocene period.

Prof. Locke read a paper "on a new species of Trilobite, found at Cincinnati, Ohio," and called by him Isotelus maximus.

This species is characterized by its elliptical terminations, and by a thorn-like process about one tenth of the length of the animal, projecting backwards from each angle of the shield, similar to an Ogygia. He exhibited casts of one entire specimen, nine and three fourths inches in length, and of a fragment of another of double that size in linear dimensions, which of course must have been nineteen and a half inches long—the largest specimen hitherto known to have been found.

Dr. Jackson stated that trilobites had been found in the limestone at the mouth of St. Croix river. He then exhibited the following specimens of minerals and fossils, viz.

Fossils from the limestone belonging to the red sandstone group of Machias, Maine. A new mineral from Unity, New Hampshire, which he has analyzed and proposes to describe under the name of chlorophyllite; it was remarkable as containing a large amount of phosphoric acid. A new mineral from Natick, Rhode Island, described by him under the name of Masonite. Tin ore from Jackson, New Hampshire, near the celebrated gorge of the White mountains. Phosphuret of copper and iron mixed with

tremolite, from the town of Warren, N. H.; the mass yields from 6 to 12 per cent. of metallic copper. Recent bituminous coal from the vicinity of Newfield, Maine, taken from a peat bed. New red sandstone from Tobig river, in New Brunswick, containing about one half its weight of gypsum. Syphonia, a fossil-like substance with tubulæ running through them and assuming various forms. They were supposed to be concretions formed around twigs and roots of trees or other organic matter.

Dr. L. C. Beck read a paper "On the Sulphur Springs of the State of New York."

In this paper the author noticed, 1st. The geographical range of these springs, their geological positions and associations. Under this head it was stated that they are found in almost every formation, from the slates of the Hudson river to the shales of Erie and Chatauque county, having a range over nearly the whole state, and being found in almost every county. 2d. The amount of gaseous matter evolved by these springs. This cannot be correctly ascertained, but from many facts stated by the author, there can be no doubt of its vast quantity. Some instances were mentioned in which large streams and ponds were impregnated with sulphuretted hydrogen. It was also remarked, that independently of the amount of gas which is held in solution by the waters of these springs, there is often a flow of gas which seems to be undissolved or uncombined. 3d. Some facts were stated in regard to the uniformity in the composition of these springs. In all cases in which they have been examined, they contain, in addition to the sulphuretted hydrogen, a small proportion of carbonic acid. The solid matters are almost invariably sulphates of lime and magnesia, with smaller proportions of carbonate of lime, and occasionally sulphate of soda. It was especially observed that sulphate of iron is very rarely found among the solid ingredients of these waters. Common salt is often found in the sulphur springs which occur in the vicinity of the Onondago brine springs. 4th. The author next adverted to the observations which had been made in regard to the temperature of the New York sulphur springs. Although these have not yet been very extensively conducted, those which have been made seem to warrant the inference that the temperature of these springs is somewhat higher (say 1° to 3°) than that of the mean temperature of the localities in which they are found.

The author then proceeded to examine the theories which have been proposed to account for the formation of sulphur springs. The decomposition of iron pyrites, often assigned as a cause, was objected to on the ground that it was not sufficiently general—that it did not meet those cases in which these springs are found in the older rocks—that from what is known concerning the decomposition of iron pyrites, it seems to be in-

adequate to account for the enormous quantity of sulphuretted hydrogen which is evolved, and lastly, that the almost entire absence of sulphate of iron in the New York sulphur springs, is irreconcilable with this theory.

The author then noticed the two general theories which have been proposed in regard to the origin of these springs, and gave the preference to the chemical theory, or that which attributes them, as the products of the great volcanic focus, to a chemical agency, as most consistent with the facts hitherto observed in the State of New York. He proposed, however, to extend the chemical theory so as to include the action of water upon the sulphurets of the bases of the alkalies and alkaline earths assumed to exist in the interior of the earth.

The Association then adjourned until 10 o'clock, Wednesday morning.

Wednesday, April 7th, 1841, 10 o'clock, A. M.—The Association met pursuant to adjournment. *Prof. Silliman* in the chair.

The subject of sulphur springs was discussed by *Messrs. H. D. Rogers*, *Locke*, *M. H. Boyé*, *Johnson* and *Mather*. *Mr. Vanuxem* announced his intention of presenting his views in regard to the New York sulphur springs at the next meeting of the association.

Prof. Hubbard presented a specimen of the slate found at Waterville, Maine, containing impressions, which in the Geological Report of Maine, were described as resembling ferns and fuci, which they resembled more than any thing else that had been found at that time, and of course an error in regard to their nature was unavoidable. Having received Murchison's Silurian system about two years since, Prof. H. found that the impressions were true Annelides and belong to the two genera Myrianites and Nereites figured in that work; thus carrying the occurrence of organic life in the New England rocks, one step lower than had heretofore been observed, and showing a coincidence between the Waterville slate and the slate containing the Annelides described by Murchison and included by him among the Cambrian rocks.

Dr. Jackson observed that he had received information from other gentlemen, that impressions of ferns occurred in the Waterville slate and had stated this in his first annual report of the geology of Maine. He had, however, subsequently visited the locality and satisfactorily ascertained that the slates of Waterville do not belong to the coal formation, and this fact was stated by him in his second report. Dr. J. remarked, that in justice to the gentlemen referred to, it should be observed that Prof. Sedgwick and

Mr. Murchison's report was not then published, and consequently these fossils could not be identified with the Annelides there described. On seeing this work Dr. J. had been able so to identify them, and he now concurred in the views expressed concerning them by Prof. Hubbard.

Prof. Mather stated that he had found an entire analogy in the fossils of the slates on the Hudson river, in Rensselaer and Saratoga counties, and in the western parts of the state.

Mr. Vanuxem confirmed the statements of Mr. Mather in regard to the identity of these slates.

Mr. W. C. Redfield made some observations concerning the fossils in the flagging slates employed in the city of New York. These slates are generally obtained from the counties of Greene and Ulster, N. Y. He referred to the corner of Cedar street and Broadway, and to the walk in front of the Spring street church, near the Hudson, as exhibitions of these impressions.

Prof. H. D. Rogers observed, that the pavement in front of the United States Bank, (Philadelphia,) afforded a similar exhibition.

Dr. Jackson now offered some general remarks upon the geology of the states of Maine and New Hampshire.

At 12 o'clock the Association adjourned, as a mark of respect to the memory of General Harrison, late President of the United States, whose funeral took place at this hour.

April 7th, 1 o'clock, P. M.—The Association met, *Prof. Silliman* in the chair. After the transaction of some ordinary business,

Mr. W. C. Redfield laid on the table sundry specimens of fossil fishes found in the red sandstone formations of Connecticut, Massachusetts, and New Jersey.*

Of eight species from these formations comprised in the collection, five species are found to belong to the genus *Palæoniscus*, and three species to the genus *Catopterus*. It is remarkable that nearly all of these several species are common to most of the known localities of these fossils in the above mentioned states. The importance of this fact, as aiding to establish the cotemporaneous character of these formations, induced Mr. R. to place this collection before the Association.

Mr. R. stated that the lithological appearances of the shales in which the fossil fishes are found, as well as of the more minute and undeter-

* See Mr. Redfield's paper, published at length in the present number of this Journal.

mined fossils which they contain, are nearly alike in all the localities which he has visited in the above mentioned states. Slight contortions of the strata with small faults or dislocations, which in some cases affect the fossil specimens, are also common to the several localities, and seem to be referable to like causes.

In addition to the above, Mr. R. also exhibited specimens of a new species of *Catopterus* from the rocks which overlie the coal mines in Chesterfield county, Virginia.

Some remarks upon the elevation of trap dykes were made by the chairman, and *Profs. H. D. Rogers*, *Mather*, and *Hitchcock*. The Association then adjourned until 4 o'clock.

April 7th, 4 o'clock, P. M.—The Association met, *Prof. Silliman* in the chair.

Mr. Vanuxem, from a committee appointed at the last meeting of the Association, presented a report in regard to the "Ornithichnites or foot-marks of extinct birds in the new red sandstone of Massachusetts and Connecticut," observed and described by Prof. Hitchcock. This report confirms the opinion respecting these appearances now entertained by Prof. Hitchcock.

Report on the Ornithichnites or Foot Marks of Extinct Birds, in the new red Sandstone of Massachusetts and Connecticut, observed and described by Prof. Hitchcock, of Amherst.

The undersigned, forming the committee to whom the subject of the origin of the bird-tracks of Prof. Hitchcock was assigned, beg leave to present the following brief report.

It may be well previously to state, that the object of the meeting in appointing this committee, was founded solely upon the desire to produce, if possible, unanimity of opinion, there being a few of the members who dissented from the views, published by Prof. Hitchcock. In our country, the subject, as it undoubtedly ought, had attracted considerable attention. It had been very favorably received and republished in Europe, and from its great importance to Palæozoic geology, an attempt should be made to settle the question; for were the views of our highly respected member correct, we were made acquainted with the earliest period in which biped animals existed whose foot-marks were analogous to, if not identical with, those of the tread of birds. On the contrary, if wrong, we were presented with another class of facts, which show that certain appearances supposed to belong solely to animal life, were held and presented by the vegetable kingdom likewise.

We shall now state, in a few words, what we suppose are the general facts upon which Prof. Hitchcock's views were founded, and then the facts of those who assumed the opposite opinion.

The first and most obvious impression upon the mind, on looking at the indentations or marks, is their thin tripartite form, resembling the tread or foot-mark of those kinds of birds which show three toes, the fourth one being rudimental, and are referable to no other known kind of animal. The tracks or foot-marks in several localities are arranged in a determinate order, like those of a bird or fowl moving in a straight line, the toes or marks in all such cases being alternate; that is, if the right foot be presented on the rock, the left would next follow, and thus right and left in regular succession, sometimes with many repetitions. In other instances, the foot-marks presented no determinate direction or order, as might naturally be supposed of a bird or any other animal having no particular place or object in view.

In all cases where a succession of tracks was observed, there was an uniform correspondence as to size, and considerable regularity as to distance between the tracks. Whatever deviations were observed, they were not greater than might be supposed to take place in animals possessed of voluntary motion.

On some surfaces, not unfrequently one or more different kinds of track were exposed, belonging, as was reasonably conjectured, to different species and genera of ornithichnites.

That the slaty material of the rock showed that the impressing body possessed force or weight, for frequently the thin layers or laminæ were bent downwards for an inch or more, and that the mud of which the slate was formed was of a highly adhesive or tenacious character.

In all cases the foot-marks or part impressed, was the fixed part of the rock; the part removed when the lower side was turned upwards, shewed the cast or what corresponded with the toes or foot. That no trace of any organic matter could be perceived occupying the cavity or mould, the cast or part in relief being in all respects like the material of the rock of which it formed a part.

Finally, that the foot-marks belonged to a group of rocks which must be considered to have been produced by the same general causes which gave rise to the new red sandstone of Europe, and referable only to that sandstone. This sandstone presents foot-marks in many localties, though comparatively but a few years have elapsed since attention has been called to them. Some of the specimens have reached this country, and had they not, the information is well given by Dr. Buckland in his Bridgewater Treatise. The most remarkable of these foot-marks, is that of the chirotherium from the quarries of Hesberg, near Hildburghausen in Saxony, and greatly resembles a fleshy human hand. These, in the drawing and in the specimen which we have seen, are alternately right and left. Other foot-marks have been observed by Mr. Linse in the same sandstone, having made out four species of animals, some of which are conjectured to belong to gigantic Batrachians. Near Dumfries, the foot-marks

of animals, probaly tortoises, were obtained from the same sandstone, but as yet no tracks like those of New England have been discovered.

The facts, &c. which led to a different conclusion are these. First, that the forms assumed by fucoidal plants were numerous and imitative, some resembling the tail of a rooster, the cauda galli; another, which was like unto a large claw or paw, and which may have been a Lusus naturæ, and the two specimens on the table of the Association, which present in relief a distinct tripartite form. These, as they all appertain to rocks of great antiquity in comparison with those of New England, it appeared more reasonable to believe that there might be resemblances as perfect, as the fossils with a tripartite character were approximations to the forms in question.

That no trace of organic matter could be discovered by the eye in the greater number of the fucoides. In some, such as the Harlani, they have been seen to be made up of small pebbles, presenting no little difficulty, not to the manner only in which the organic matter was replaced, the external form being complete, but the nature of this material, which could make so definite an impression and preserve its form entire.

There were other facts which showed resemblances, such as that the part in relief was the part removed when the fucoides was attached to the sandstone at its upper part. It may also be stated, that the appendages to the heel of some of the New England tracks, might have been caused by a bird whose legs were feathered, but not to a wader, and they favored their vegetable origin, for the appendages might readily be conceived to be either leaves or radicals, or both.

From a comparative examination of the facts on both sides, your committee unanimously believe, that the evidence entirely favors the views of Prof. Hitchcock, and should regret that a difference had existed, if they did not feel assured it would lead to greater stability of opinion. To liken things to what we know is the nature of mind, the error from this tendency increases with ignorance, and diminishes as knowledge increases, so that he that knoweth all things, as is self-evident, can commit no error when following this instinct of his being. The discoveries of Prof. Hitchcock were published at a period when the mind of those who embraced the negative side of the subject was preoccupied with the anomalous vegetation with which many of the Silurian rocks of New York abound, and to which provisionally the name of fucoides had been given. From this imitative character, and from finding a few specimens presenting a tripartite or trifurcate form, &c. it appeared not only possible but probable, that the impressions from Massachusetts and Connecticut, were with greater propriety referable to fucoidal bodies, than to those which Prof. Hitchcock had assigned them.

We may here remark how essential it is that truth or the facts which make manifest any truth, should first be presented to us; so readily is the

mind impressed when not preoccupied, and when a strong impression is made, be it ever so false, it is no easy matter to free ourselves from it. From this circumstance we can readily foresee the advantage which future generations will possess over those of the present and especially those of former times. As the progress of knowledge is certain, each day will lessen error and enlarge the domains of truth, and should man be true to his permanent interests, error finally will cease to have existence.

Signed, HENRY D. ROGERS, LARDNER VANUXEM, RICHARD C. TAYLOR, EBENEZER EMMONS, T. A. CONRAD.

Mr. Vanuxem read a paper "On the Ancient Oyster Shell Deposits observed near the Altantic coast of the United States."

Among the unsettled subjects of geology in our country, is the origin of the deposits of oyster shells, (*Ostrea Virginica,*) observed in many parts of the Atlantic seaboard, of which a few only of those near South Amboy have come under our notice. But the greater number of those of the largest dimensions are in the waters of the Chesapeake. Some of these southern deposits of shells are enormous, covering, it is said, acres of ground, adding no small weight to the truth of that belief that considers them *in situ*, as ancient oyster beds, raised from their original position by the uplifting of our coast, of which the fact of their generally holding, if not a real, an apparent similarity of level would seem to be ample confirmation. This theory of their being in place, is highly satisfactory, being in accordance with the less modern deposits beneath them, adding one more to the number of elevating movements to which our coast has been subjected, thus mutually confirming each other, making the certainty of these movements sure.

When the nature of their origin was advanced by Mr. Conrad, I confessed a decided bias; for I knew not the facts upon which Dr. Ducatel, the geologist of Maryland, maintained the opposite one. None were known to me adverse to the views of Mr. Conrad, for the history of our country afforded no light that could be recollected, either as to the origin of these oyster shell deposits, or to any extraordinary manifestation of gastromic power in the aborigines, in respect of this article of diet, which would lead me to infer their existence, and which the magnitude of some of the deposits required.

The eastern shore of Maryland presents many deposits of these oyster shells, hitherto until recently, unused and little examined, so far as knowledge has been received this way. Now, as many of the planters in that section of the country are waking from the deep slumber of the past, and turning their attention to the all-important subject of improving their lands by the use of lime, a few of these deposits have become the subject of investigation, furnishing facts, which, were the same discovered elsewhere, would settle the question of their origin, and in favor of the Maryland geologist.

At the mouth of Pickawaxent Creek, about eighty miles below Washington, there is an extensive deposit of oyster shells, at which an establishment has been formed, which, in a few months has converted many thousand bushels of them into lime. Before any excavation of the mass was commenced, I had directed the attention of Mr. Downing—one of the partners concerned—to the doubtful nature of their origin, requesting that all facts tending to throw light upon them should be carefully observed and preserved. When Mr. D. first went into the country, he was in favor of the views of Mr. Conrad; it was only by the examination of the mass at the Pickawaxent, of another not remote from that one, and from subsequent observation in the city of Baltimore, showing the amount of shells which there accumulates, was he assured that their origin was to be referred to man, and not to other or more elementary powers of nature.

The first and most important fact there observed, was that neither he nor any of the hands employed in getting out the shells had been able to find any two valves which fitted each other, excepting in one instance; a waterman having brought the specimen to him. The deposits having the nature of a mass or heap composed of shells whose valves were separated before being thrown together.

That in many parts of the mass arrow-heads and fragments of pottery have been found in the progress of excavation—these in no wise different from those found in old settlements of the Indians.

That the bottom of the bed is formed of the yellow loam or soil of the country, and that the roots and other parts of the cedar of the country have been met with at the bottom of the bed, showing a growth upon the surface, before the shells were deposited upon it.

That these deposits are at the mouths of the creeks, extending up the creeks, and rarely extending along the river shore, owing, as Mr. Downing conjectures, to the excellent fishing which the creeks furnish, and which would give to those who accumulated the shells, a twofold advantage.

That the shore is low on that side of the river where they are found, and the recent oyster in great abundance on that shore, whilst the channel is on the Virginia side, and no deposit of oyster shells existed in that section of country.

That these deposits are of some comparative antiquity, is to be inferred from the soil which is found upon them, and the existence of an exceedingly old cedar growing upon the top of a mass, and from the silence of history or tradition respecting them.

Against these facts which show an undoubted human origin for these deposits of oyster shells, there are others cited by Mr. Conrad which he has made me acquainted with, since this paper was written, which either I had not known or they had escaped my memory, and are equally con-

clusive as to the opposite opinion. The facts are, that masses exist composed of whole shells, as at Easton, on the eastern shore of Maryland. That in some localities fragments of older fossils are found with them, and which must have been thrown amongst the oysters by the waves of the estuary from their position below. And again that deposits of the shells exist in situations too remote from present oyster beds to have been removed by human agency, such as those in Cumberland county, in New Jersey; therefore it would appear that both causes have operated to produce them, and that no single generalization can comply with the requisition of the facts which they present, leading as they do to a twofold, one from opposite conclusions, one referable to human, the other, to natural causes, and that severally they must be examined in order to ascertain to which of the two causes any given mass is to be referred.*

LARDNER VANUXEM.

Prof. Booth stated that his observations upon these deposits had led him to the same conclusion as that which had been arrived at by Mr. Vanuxem, viz. "that they are sometimes referable to human agency, and at others to natural causes." In answer to an inquiry, Prof. B. observed that these shells reduced to powder had been used with great success in the State of Delaware, as a manure. *Prof. Hitchcock* stated that the fertilizing powers of these deposits of shells had also been tested by experiments on Cape Cod. Facts were stated by the chairman and *Prof. Mather* in regard to beds of oyster shells similar to those described by *Mr. Vanuxem*, on the Island of Nantucket, and on Long Island.

Prof. Bailey commenced his account of "Fossil Infusoria," by an exhibition and description of the microscope employed by him in his researches.

Messrs. Charles B. Trego and *B. Silliman, Jun.* were appointed assistant secretaries. The Association then adjourned until 9 o'clock, Thursday morning.

Third day of meeting, Thursday, April 8, 1841.—The Association met at 9 o'clock, A. M. *Prof. Silliman* in the chair. The minutes of yesterday were read and adopted.

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* Since the meeting of the Association, I have found on conversing with Dr. Ducatel that the impressions which I had of his views were founded upon his first report, that of 1834, for in his subsequent ones, he makes known their twofold origin. We should withdraw this paper did we not believe that it would be of service; for it not only settles the point in question, which was its object, but it affords a useful lesson as to caution in an expression of the opinions of others.

Peter A. Browne, Esq. laid on the table, for the inspection of the members, a suite of specimens, chiefly fossils, from the chalk basin of Paris, collected and labelled by A. Brongniart.

Prof. Locke made some observations concerning the connection of magnetism with geology, mentioning an instance where he found an increase of the dip and intensity as he approached, from south to north, a certain point or meridian line, and a decrease as he receded from it; also remarking that a similar change is found upon crossing the Ohio river: querying from this, whether the water of large streams running east and west, has an influence on the magnetic relation.

Dr. Houghton remarked, that in the vicinity of the great northwestern lakes a change in the magnetic deflection was frequently found on approaching within a few miles of a large body of water.

Dr. R. E. Rogers called the attention of the Association to the subject of limestones, observing that he thought the magnesian character of these rocks generally had not received sufficient attention. He stated that he had found, upon analyzing some of the lower limestones of Pennsylvania, a larger proportion of magnesia than is requisite for the formation of a true dolomite, and threw out the query as a point of scientific interest, whether the carbonate of lime and carbonate of magnesia were chemically combined in the proportions to form dolomite, and this mingled throughout the excess of the carbonate which might be present, or whether the two carbonates were mechanically and uniformly intermingled.

Dr. Jackson stated, that he considered the granular or crystallized dolomite to be a regular chemical double salt, consisting of one equivalent of carbonate of lime and one equivalent of carbonate of magnesia. But he had never found any magnesian limestone to contain *more* than this proportion of magnesia, although he had frequently analyzed limestones containing a *less* proportion than one equivalent of magnesia. His published analyses will illustrate this remark.

Dr. J. inquired whether Dr. Rogers had ascertained if the limestones to which he alluded did not contain the hydrate or silicate of magnesia, mixed with dolomite. If the rock was of the compact variety, this might have been the case.

Dr. James Rogers thought we must consider dolomite as a true double salt: 1 atom carbonate of lime + 1 atom carbonate of magnesia; the excess of magnesia found in our limestones must be considered a mechanical mixture.

A communication was received from *Peter A. Browne, Esq.*, expressing a willingness to read before the Association "an Essay on Aërolites or Meteoric Stones," at the next annual session. Laid upon the table.

Prof. Mather made a verbal communication on the joints in rocks, particularly as they occur in the primary, transition and secondary of this country. He found two principal sets of joints prevailing; the first had a general direction of north by northeast, the second set were nearly perpendicular to the former—besides these, there were two other sets not so well defined. The joints in the primary were not so smooth and well marked as in other formations; this observation was not intended to apply to the joints of slate rocks.

Dr. Jackson cited the joints or fractures of the conglomerate around Boston, and particularly at Roxbury, Mass., and also in the island of Rhode Island, at a place called Purgatory, the large pebbles are broken by these fractures, without dislocation or loosening from their beds. He supposed the parallel and uniform cracks in the lime rocks and slates of that country to have connection with the different epochs of irruption of the trap, granite and porphyry.

Dr. Douglass Houghton inquired of Dr. Jackson if these cracks in the conglomerate had reference to the line of bearing, remarking that in Michigan they were nearly at right angles to the line of the longer diameter of the pebbles. Dr. Jackson replied that such was the case in the cases he had cited; that at Purgatory the pebbles were very large, ovate, and arranged with their longer diameters in one direction, and seemed to be joined together by very little cement, and yet they were broken at right angles to their longer diameter, without dislocation. He stated that Mr. A. A. Hayes had found that chloride of calcium would concrete pebbles of quartz into a firm mass—this fact might elucidate the present subject. Specular iron was generally observable among the interstices of the pebbles at Purgatory, and more or less of iron and lead ore was generally to be found at the juncture and fissures of the trap dykes.

Prof. Hitchcock thought that the steps of the new red sandstone of the Connecticut valley were the result of the fractures referred to by Prof. Mather—they were nearly coincident with the strike of the strata, as if caused by elevatory movements. He found difficulty in distinguishing between fissures produced by mechanical violence and joints properly so called; he viewed those of the conglomerate as mechanical, those of the slates as chemical. Two cases occurred to him as worthy of notice; the first was of a dyke of greenstone crossed by parallel transverse planes two or three feet apart and at right angles to the strike of the vein. The second case seemed to throw some light on the origin of this class of phenomena; it occurred in a bed of the common blue diluvial clay—the horizontal layers were unmoved, but some of them were divided into double rhombs. The experiments of Mr. Fox, of England, in the lamination of clay by galvanism, seem to explain this structure.

B. Silliman, Jr. had found this rhombic structure in great perfection in the argillaceous sandstone of the Connecticut valley at Hartford, in Connecticut, where this variety of sandstone is used for flagstone. Many of the joints parallel to this rhombic structure are filled with carbonate of lime.

Dr. C. T. Jackson stated that the great trap dykes of Nova Scotia had the perpendicular columnar structure in a high degree—in the smaller dykes this structure prevails from side to side of the dyke, perpendicular to the walls. He thought that in all cases these phenomena were referable to the way in which the dykes cooled—the structure being perpendicular to the cooling surface—thus the narrow dykes cooled from side to side, and the heavy ones from the upper surface downward.

Prof. Henry D. Rogers remarked, that the trap dykes of Pennsylvania and the magnetic iron ore of New Jersey were abundantly characterized by the columnal structure. He viewed the horizontal columnar structure of the magnetic iron ore, as a very important indication of its igneous origin.

This discussion was here suspended to give an opportunity for Prof. Bailey to read his paper on recent and fossil infusoriæ.*

* As this paper of Prof. Bailey is to appear in full, with plates, in the October No. of this Journal, it has been thought unnecessary to attempt here any abstract of his remarks before the Association.—B. S. Jr.

Dr. Jackson stated that the mass of infusorial deposit found under peat bogs is hydrate of silica, which loses by being heated to redness from 12 to 15 per cent., principally vegetable matter. Great abundance of this material occurs at Newfield, in Maine, where it covers many hundred acres, and is five or six feet thick. After burning, it is so white and beautiful that it has been fraudulently sold for magnesia alba. The ammonia which is evolved in its destructive distillation, is probably derived from the crenic and apocrenic acids which it contains. Phosphate of lime and manganese are found in it in small quantities. As a fertilizer of land, it is considered of use when containing in large quantity the juices of plants.

A memoir from M. Alexandre Vattemare was presented, proposing a general system of exchange of objects of nature and art among all nations. It was by motion laid on the table.

The subject of bowlders and diluvial scratches was then brought up for discussion by Prof. Mather, and a protracted debate ensued, in which many of the members joined.

Prof. Mather inferred from the facts in the case, that the bowlders and diluvial scratches had, in general, come from the north; those on the east of the Hudson from the northwest, those on the west from the northeast, as by the result of two forces.

The diluvial furrows are, in general, parallel to the valleys in which they are found—thus in the small transverse valleys, the scratches are found parallel to the direction of the valleys—and not coincident with those of the main valleys. All the bowlders seem to have been brought from the northwest, both at the east and beyond the river St. Peter's at the west, and very few are found below 38° or 39° of north latitude.

The chairman (*Prof. Silliman*) cited the recorded observations of Mr. C. Darwin, naturalist to H. M. ship Beagle, that in South America no bowlders occur nearer the equator than about 40° south latitude.

Prof. Mather had not seen any bowlders in the coal region of Ohio, and very few in Kentucky. He thought that the bowlders mentioned by Mr. Hodge in the gold region of North Carolina, were not transported masses, but were composed of granite which had suffered decomposition *in situ* by atmospheric agency.

Prof. Henry D. Rogers said there was need of much caution in the use of the term bowlder, as regards the size of the mass to which it should be restricted; he was inclined to give the term much latitude. Thus he conceived that a current of drift coming from the north and meeting the terraces of Pennsylvania, would there be arrested and deposit its larger masses—and so from stage to stage, until the onward current would carry forward only the smallest sand; in this way, we may find among the drift of the south, all the materials derived from the northern rocks.

He concluded that all the materials of a current of drift, find their resting place in accordance with gravity.

Prof. Mather doubted whether the large bowlders found in Long Island, resting on beds of sand or fine gravel, could be thus accounted for, because a current of sufficient force to move such large masses would have carried away the sand.

Prof. Rogers replied, that diluvial action could not be restricted to a single epoch.

We must find in secular and periodical elevation, the cause of the translation of the beds of infusorial earth recently found in the tertiary of Virginia, which are there covered by the quiet strata of the Meiocene. We have evidence of numerous slight elevatory movements on the eastern coast of North America, and the various terraces of our rivers seem to present the same phenomena; for the source of these elevatory movements we must look to the great volcanic foci of Greenland.

Prof. Locke mentioned a locality in Ohio, at which the limestone is ground down to a perfect plane, as if it had been done by a stone-cutter by grinding one stone on another, over an extent of ten acres. Upon this planished surface, lines have been engraved in systems perfectly straight and parallel, running from northwest to southeast. Some of these lines are fine, as if cut with the point of a diamond, and others perhaps half an inch broad, and one eighth of an inch deep, scaled rough in the bottom, as if they had been ploughed by an iron chisel properly set and carried forward with an irresistible force. Prof. L. inferred from the facts of the exact straightness and parallelism of these lines, that they had been formed by a body of immense weight, moving with a momentum scarcely affected by the resistance offered by the cutting of the grooves. Such a momentum and

actions would be supplied by a floating iceberg, whose lower surface should present projecting sharp points of imbedded bowlders.

Prof. Mather further stated, that the bowlders of Ohio were in continuous lines and groups, and not scattered promiscuously. On the river St. Peter's, the bowlders may be seen extending for miles, as along a coast line; in some situations one might see them bounding the horizon as far as the eye could reach.

Dr. Locke, in conformity with Lt. Mather's statements, mentioned a region of bowlders in Ohio, extending from the town of Eaton quite across the state; five miles in width and over forty in extent.

Dr. C. T. Jackson remarked, that the phenomena of diluvial currents were well exhibited in the vicinity of Providence, at Cumberland, R. I. A large mass or mountain of porphyritic titaniferous iron of very peculiar character exists in that place; to the north of it no bowlders are to be found, but on the south, huge bowlders of it may be seen, and so abundant that the stone walls are built of them; and below, at Papoose Squash Neck, small bowlders of the same characteristic rock are found; south of Newport, and still further south, the same are met with of a smaller size, the whole extending from north to south forty miles, and from six to fifteen in width, diverging to the south.

The characteristic macle rock at Lancaster, Mass., presents similar phenomena, being found in loose masses to the south as far as Bolton, while none can be found to the north of the locality.

He considered the power of the diluvial currrent greater to the north than at the south, since the evidences of it in Maine are much greater than in Rhode Island; bowlders have been found on Mount Katadin as high up as four thousand feet; he thought there was no evidence of any elevation of the rocks after the diluvial current had passed.

Mr. Nicollet proposed, at a future meeting, to make some remarks upon, and to exhibit specimens from, the cretaceous formation on the upper Missouri.

Mr. Redfield expressed a wish that the attention of the Association should, at some convenient time, be called to the recent sand formation along the eastern coast of the United States.

Adjourned to meet this afternoon at 4 o'clock, at the rooms of *Mr. Richard C. Taylor*, for the purpose of viewing a model of

the coal region of Dauphin and Lebanon counties, east of the Susquehanna.

Thursday, 4 o'clock, P. M.—The Association assembled at the rooms of Mr. Taylor, where that gentleman exhibited a highly interesting model in plaster of the Dauphin and Lebanon coal region, embracing, altogether, an area of seven hundred and twenty square miles, showing the range of the mountain elevations, with their relative height and position; also their elevation above tide level; the dip of the rocks, the position of the coal seams, and much other useful information.

Mr. Taylor accompanied this exhibition with remarks explanatory and statistical, in relation to this coal region, and made some observations on the importance of this mode of exhibiting the geological features of a country, expressing the hope that the day would come when models of this kind, representing the several states, and even the whole United States, shall be constructed. He also enlarged upon the propriety of following, as closely as possible, the actual conformation of the country in drawing sections, and of adopting uniform modes of illustration by colors, &c., and the importance of an equal scale of extension and elevation as far as practicable in such sections.*

Prof. H. D. Rogers followed with observations upon the Pennsylvania coal formations and the range of their underlying rocks, detailing what he conceived to be the cause of the inverted dip observable along the southern border of the Kittatiny series, ascribing it to a great force acting laterally, and folding and crushing the axes so as to produce this inverted dip by tossing the strata many degrees beyond the perpendicular, and thus producing the present apparent dip of the lower stratified or sedimentary rocks *beneath* the primary.

Adjourned to half past nine o'clock to-morrow morning.

In the evening, the members of the Association had the pleasure, in common with a number of citizens, of listening to a very interesting and appropriate address from Prof. Hitchcock, embracing all the points at present most interesting to the American geologist.†

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* See Mr. Taylor's article published entire, with a colored section, in the present number of this Journal.—B. S. Jr.

† As this address is to be published entire by the Association, and it is intended to give an abstract of it in the next number of this Journal, no farther notice of it is inserted here.—B. S. Jr.

Fourth day of session, Friday, April 9, 1841.—The Association met, pursuant to adjournment, at half past nine o'clock, A. M. *Prof. Silliman* in the chair.

After the minutes of yesterday had been read and adopted, *Dr. Beck* moved a series of resolutions, of which the first was adopted, as follows:

Resolved, That the thanks of the Association be presented to Professor Hitchcock for the interesting and valuable address delivered last evening; and that a copy of the same be requested for publication.

The committee on business reported the following resolutions, which were all adopted excepting the first,—it being laid on the table.

Resolved, 1. That the committee recommend to the Association the first Monday of May as the period for the next annual meeting.

2. That the Association adjourn its present annual session this week.

3. That a committee of five be appointed to draft a constitution and by-laws for the regulation of future proceedings of the Association, and that each member of the committee be recommended to draft a plan of organization, to be discussed by the committee.

4. That at each meeting a local committee of three members, resident at the place of the next annual meeting, be appointed, for the purpose of making arrangements for the reception of the Association.

5. That the members of the "Academy of Natural Sciences" be invited to attend the present session of the Association, and to participate in its proceedings.

Prof. Renwick, *Mr. Nuttall*, and *Dr. Hayden* of Baltimore, were recommended as members of the Association.

Dr. Harlan exhibited models of the fossil remains of the *Dinotherium giganteum.*

The first specimen presented to the view of the Association, was the cast of a small model of the *Dinotherium giganteum* or the great fossil *Tapir* of Cuvier—the only model of the kind, which, as far as Dr. Harlan is aware, has yet reached America. The Paris Garden of Plants, possesses a model of the skull of the size of nature, which is sold by the German naturalists, Messrs. Klipstein and Kaup, for $100. The dimensions of this skull are four feet in length, three feet in width, and two feet in height. In peculiarity of structure and colossal dimensions, the

Dinotherium constitutes one of the most curious and interesting animals of an antediluvian Fauna. M. Klipstein, Professor in the University of Giessen, a few years since, discovered a perfectly preserved specimen of the skull on the borders of the Rhine. Baron Cuvier had many years previously described, in his Fossil Animals, some remains of this animal as allied to the genus *Tapir*. The fragments subjected to his observation consisted only of two imperfect pieces of the lower jaw, and some molar teeth. From such data alone, he was able to represent them as belonging to two distinct species, *Dinotherium giganteum* and *D. Cuvieri*, and to estimate the size of the larger species at eighteen Paris feet, which was subsequently proved to be correct. In 1829, Mr. Kaup, director of the museum at Darmstadt, discovered and described numerous portions of this animal, all obtained from the same strata of the tertiary sand of Eppelsheim.

The whole animal creation, fossil or recent, presents no parallel to the structure of the lower jaw and tusks of this animal. The anterior portions are recurved downwards, and from which depend two enormous tusks, in a direction downwards and backwards. The upper jaw is destitute of incisors. The configuration of the anterior nares and their vicinity, demonstrates that the animal was supplied with a *proboscis*, and like the hippopotamus and tapir, the habits of the animal were evidently aquatic; and the peculiar arrangement of the tusks was evidently adapted to the nature of the animal's food and the means of attaining it—they would be very useful in unison with its powerful claws, in eradicating from the mud the thick and succulent roots of aquatic plants, which probably constituted its principal nourishment. A correct notion of the enormous dimensions of this animal may be obtained by a view of the models of the tusk after nature, as well as by a series of the molars of one side of the lower jaw. It evidently attained a size far exceeding that of the hippopotamus of our day.

The last or ungual phalange, presents so close analogy to that of the Manis or scaly ant-eater, that Cuvier, at first sight, referred this species to an animal of that genus, and named it *Manis gigantea*. In offering you my own views of this peculiar specimen of a departed type, it should be stated that various notions exist among different naturalists, as to the real nature and habits of the animal in question. Some German naturalists place it among the *Phocæ*. Blainville took it for a pachydermatous animal, closely allied to the elephant. Kaup considered that it might range as a fifth and last family of the class Edentata. Others referred it to the herbivorous Cetacea, &c. &c.

Dr. H. also made some observations upon the remains formerly described by him as belonging to the "Basilosaurus," but which he is now satisfied, from the microscopic examinations of a section

of one of the teeth by Prof. Owen, should be referred to a genus of the aquatic mammalia, and which is now named "Zygodon," —specimens of the vertebræ of which, from the tertiary deposits of Alabama, he exhibited to the Association.

Mr. Nicollet then made some highly interesting remarks upon the geology of the region on the Upper Mississippi, and the cretaceous formation of the Upper Missouri.

He referred to his arrival in this country for the purpose of making a scientific tour, and with the view of contributing to the progressive increase of knowledge in the physical geography of North America. After spending several months in Philadelphia, Baltimore and Washington, he proceeded through the southern states; explored the south Allegany range, the states of South Carolina, Georgia, Kentucky, Mississippi, Alabama, Florida, Louisiana, Arkansas Territory, and Missouri; ascended the Red River, Arkansas river, and to a great distance the Missouri river. Having thus made himself well acquainted with the lower half of the Mississippi, he undertook the full exploration of that celebrated stream, from its mouth to its very sources; the latter of which he succesfully reached near the close of the month of August, 1836. During five years of unremitted exertions, he took occasion to make numerous observations calculated to lay the foundation of the astronomical and physical geography of a large extent of country, and more especially of the great and interesting region between the Falls of St. Anthony and the sources of the Mississippi. With these labors was connected the study of the customs, habits, manners and languages of the several Indian nations, that occupy this vast region of country.

Mr. N. acknowledged, in feeling terms, the generous hospitality, on the part of our citizens generally, of the agents of the American Fur Company, the civil and military officers, as well as the kind protection of the government, extended to him on all occasions, so as greatly to facilitate his operations and second the accomplishment of his designs. At the expiration of this long and arduous journey, Mr. N., broken in health, and his means exhausted, returned to Baltimore, where he soon received a flattering invitation from the war department and topographical bureau to repair to Washington. The result of his travels having been made known to these departments and appreciated by them, he was intrusted with the command of a new expedition, to enable him to complete to the greatest advantage of the country, the scheme which he had himself projected in his first visit to the far west; namely, the construction of a map of the region explored by him. This map having been recently submitted to Congress, the senate of the United States has, unanimously, ordered its publication under the direction of the topographical bureau. It is to be accompanied by a report embracing an account of the physical geog-

raphy of the country represented, together with the most prominent features in the geology and mineral resources of other sections of our western states not embraced within the limits of the map.

Mr. N. then went on to give a succinct account of his geological researches, which, modestly disclaiming any pretensions to be considered a professed geologist, he had felt an irresistible inclination to engage in, as a subject of general and growing interest. This account he offered as a more appropriate theme, in view of the objects contemplated by the present meeting.

Mr. N. said he had traced a magnesian limestone—the cliff limestone of Dr. Owen—which is probably referrible to the mountain limestone of European geologists, over a vast extent of country, within the valley of the Mississippi. Connecting his own researches with the facts brought to light in the survey of the Iowa and Wisconsin Territories by Dr. D. D. Owen and Prof. John Locke, and with the observations of Dr. Henry King, during an exploration of the country watered by the Osage river, Mr. N. thought himself warranted in assigning the Falls of St. Anthony on the Mississippi river, as the northern limit of this formation, which to the west, extends to Fort Leavenworth on the Missouri river, and to the south, embraces the metalliferous region of the state of Missouri. This limestone, containing *trilobites*, *catenipora*, and other coralline fossils, is the metalliferous rock not only in Missouri, but in Iowa and Wisconsin, from which the lead and copper ores are extracted. The rock intervening between it and the coal formation is characterized by the occurrence of the *Pentamerus oblongus*. In this relative position, also are found thin beds of oolitic limestone, that are perhaps referrible, geologically, to the oolitic limestone of Tennessee, described by Dr. Troost, who indicates the *pentremites* as their characteristic fossil; a large number of these fossils, in a loose state, was collected in the vicinity of these rocks. Shallow coal basins frequently occur in Missouri and the south part of Iowa Territory; but on the Mississippi river, the coal disappears, about thirty miles above St. Louis; thence, ascending the river as far as the great Platte river, the cliff limestone and the coal rocks present themselves in alternate succession. In the vicinity of the Platte river, as well as at Council Bluff, a limestone containing *cyathophylla* of large size, *encrinites*, and other fossils, appears in a position seemingly between the cliff limestone and the coal. Near the confluence of the Sioux river and the Missouri, there occurs a formation overlaid by a thick deposit of clay, containing, in abundance, several species of *ammonites* and *baculites*, *belemnites*, *inocerami*, &c. &c., beautifully raised on their exterior and sparry in their interior. Some of them were exhibited to the meeting. These fossils were identified with similar ones belonging to the green sand deposit of New Jersey, a member of the chalk series; but no true chalk or flint (silex pyromage) was observed. The occurrence of this formation had

already been indicated, by some fossils that Lewis and Clark and Mr. Thomas Nuttall had brought along with them from their travels, and which were described by Dr. Morton. Mr. N. exhibited farther, some fossil bones which had been submitted to the inspection of Dr. Harlan, who describes them as belonging to *vertebræ* of a Squalus and of a nondescript crocodile, also articulated vertebræ of an animal referrible to the order *Enalio-sauri* of Conybeare. The surface presented by a transverse section of these vertebræ, Dr. H. thinks peculiar, as also the mode in which the ribs are attached to a small process in the middle of the inferior surface of each vertebra. From their size and unique character, it is quite probable that these vertebræ form a part of the skeleton of the *Sauro-cephalus lanciformis*, (Harlan,) an animal possessing still more of the fish than the lizard, than exists in the organization of the *ichthyosaurus*, in which respect these vertebræ correspond. According to Dr. Harlan, similar fossils have been found in the green sand of New Jersey and in the chalk of England.

Mr. Nicollet concluded by remarking, that he had followed up and described this formation, along an extent of upwards of four hundred miles, and from information received and from fossils that had been furnished to him, thinks that it extends to the west at least as far as the sources of the rivers Running Water, White, Shayeune, &c. and northwest along the Missouri probably to the Yellow Stone, being an extent in length of about one thousand miles.

Mr. Hodge followed with some observations concerning the secondary and tertiary deposits of the Carolinas.

The remarks of Mr. Hodge regarding the secondary and tertiary deposits of the Carolinas, will be found embodied in the next number of this Journal. He next noticed the deceptive appearance of the bowlders of quartz and primary rocks, scattered over the country north of Columbia, S. C., and extending throughout the gold region of North Carolina, all *seemingly* referrible to a similar cause with that which covered the hills of the northern states with their bowlders. But according to the previously expressed opinions of Messrs. Vanuxem and Mather, these are considered not to have been transported to any distance, but to belong to the rocks in their immediate neighborhood.

He asked attention to the subject of the deposit gold mines; whether these were not still in progress of formation, notwithstanding the opinions to the contrary found in many of the foreign treatises; mentioning their occurrence always near the veins of the ore, and of the fact of veins having been discovered by working the deposits up to them, above which the gold suddenly ceased. Of the power of the freshets, the discovery of the little buried village in Nacochee Valley, Ga., was mentioned as a remarkable evidence. His opinion was, that though many of the deposits referred themselves far back to the period when the whole country was

overspread with diluvium, still that the deposits have been going on ever since. Specimens were shown from, and some remarks made, concerning the gold and copper ores of Davidson and Guilford counties, N. C. Veins originally worked for the former, gradually passed into lodes of sulphuret of copper and iron, though these formed a very small part of the veins at the surface. Rich specimens of the double sulphurets from the Harlan mine, Guilford county, were exhibited, in which mine the lode is over ten feet thick, or the depth of one hundred and five feet, and consists almost entirely of these ores.

Some account of King's silver mine, Davidson county, was given, and specimens of the varieties of the silver ore shown. The mine was originally worked for lead, the ore being a carbonate, very rich, and in beautiful crystals. Native silver was discovered, and the pig lead already made, found to contain a considerable amount of that metal. Phosphate of lead, copper, zinc and sulphuret of iron, were also mentioned as occurring in the lode, which was twelve feet thick. Some of the ore was of a soft light magnesian character, and though its specific gravity could not be twice that of water, yet it was considered a rich silver ore.

The lode lies between granite and a magnesian rock above. All the metalliferous veins, it is believed, are found at the point of contact of these two rocks.

Peter A. Browne, Esq. presented to the Association a section of the rock strata on the Schuylkill above Philadelphia, drawn about the year 1825, being the first geological section made in the state of Pennsylvania.

Dr. Houghton then made some remarks upon the subject of the metalliferous veins of the northern peninsula of Michigan.

He began by remarking, that that portion of Michigan lying between Lakes Huron and Michigan on the south, and Lake Superior on the north, is known as the upper or northern peninsula, while that portion of the state lying south of the Straits of Mackinac, is more usually known as the southern or lower peninsula.

The rocks of the easterly portion of the upper peninsula, for a distance of one hundred and fifty miles, consist of a series of fossiliferous limestones and shales, resting upon sandstones, the whole dipping a few degrees east of south. The limestones appear only on the southerly portion of the peninsula, while the underlying sand-rocks form the immediate coast of Lake Superior.

At a point very nearly one hundred and fifty miles west from the easterly extremity of the peninsula, and near to the immediate coast of Lake Superior, several low ranges of granitic hills make their appearance, which hills are flanked on the south, by quartz rock, alternating with

mica, talcose and clay slates. These hills have a general easterly and westerly direction.

Northerly from these, other ranges of hills occur, having a similar direction, but in the several ranges as we proceed north, the granitic character becomes less and less perfectly defined, being first sienitic, after this altered sienite, and finally the outer or northern range is made up of well defined trap. This range of trap hills continues very nearly unbroken for a distance of one hundred and thirty five miles within the limits of Michigan.

The trap rock, which chiefly appears as a compact greenstone, is nevertheless, quite uniformly bounded on the north by an amygdaloid, against or upon which rests a very coarse conglomerate, and upon this a series of alternating strata of conglomerate and sandstone, the whole being capped by an extensive formation of red sandstone.

The group of stratified rocks referred to, which have an entire thickness of several thousand feet, dip very regularly, and usually at a high angle, into the basin of Lake Superior; and since the same is the fact in regard to the rocks upon the north coast, that lake may be said to occupy a synclinal basin.

After some remarks upon the successive elevation of the several ranges of hills referred to, together with the long intervals of time that would appear to have elapsed between the several uplifts, Dr. H. proceeded to say, that with our present imperfect maps, it would be nearly impossible to convey a clear conception of its geographical geology, and that in fact he had made these references, only to render more intelligible what he wished to say upon the subject of the metalliferous veins of the district.

It is a fact well known, that south from the district referred to, transported masses of native copper are occasionally met with, in the diluvial deposits which are so abundantly spread over the country; and these loose masses are distributed over an area of many thousand miles, including southern Michigan, Wisconsin, Illinois and Indiana. In northern Michigan they are still more frequently met with.

The great transported mass of native copper on the Ontonagan river, so frequently alluded to by travellers, and which he, Dr. H., estimated to contain about four tons of native metal, was stated to have all the characters of the other loose masses referred to.

The source of these transported masses has, heretofore, been somewhat obscure, although there has been good reason to believe, that most of them had their origin from the trap rocks, but whether from true veins or from the mass of the rock itself, was not known. He said that after examining the country with care, he was enabled to state, that without doubt a very considerable portion of them had their origin from what may be regarded as true veins.

Those which were regarded as true veins, were uniformly noticed to originate in the trap rock, but they were frequently traced across the superimposed sedimentary rocks, to and including the red sandstone. The direction of the veins across the upper rocks most frequently corresponds to the dip of those rocks.

Dykes of trap, traversing the conglomerate and sandstone, were stated to be of frequent occurrence; but these dykes very rarely cut *across* the strata of the upper rocks, or in other words, they mostly occupy places corresponding to the lines of stratification, for which reason the veins referred to, cut across the dykes at very high angles.

So far as we are enabled to judge from the examinations which have been made, those veins originating in the outer range of trap hills are the only ones in the district deserving the name of metalliferous veins. Not only do the separate veins vary from a mere line to several feet in thickness, but those traversing the several rocks above the trap, are usually very much expanded in their passage across the upper rocks.

By far the most important minerals contained in these veins are the several ores of copper. The metal occurs in a native form associated with the grey and red oxides, carbonate and silicate, together with several mixed compounds. Sulphuret of copper is exceedingly rare, and pyritous copper has not been found in what was regarded as a true vein, though this last named mineral, associated with the sulphuret and carbonate of lead, was noticed in small ramifying veins, in what may perhaps be regarded as a distant portion of the range under consideration. Native silver was very rarely seen in the form of specks and strings associated with the native copper.

Most of the ores of copper occur in the greenstone, amygdaloid and lower portions of the conglomerate, or at points in near proximity to the dykes before referred to, and they are most abundant at, or near to the junction of the trap and conglomerate, or in immediate vicinity of the dykes, thus following the general laws respecting the deposits of the metallic minerals.

As the veins recede from the trap, the place of the copper is frequently supplied by the silicious oxide and carbonate of zinc, together with calcareous spar, which latter usually fills the entire vein in its passage across the sandstone.

The veinstone in those portions of the vein most rich in the ores of copper is chiefly quartz, and this is frequently filled with minute specks and filaments of the native metal.

Dr. H. conceives these to be veins of sublimation, or in other words to be simple fissures filled from below by the metal in a vaporous state, and that all the compounds had their origin from copper in a native form. The conglomerate was stated to have been noticed where the cement consisted

to a large extent of ores of copper, and even of copper in a native state. This was observed only in close proximity to considerable veins.

The veins, as well as different portions of the same vein, are very variable in their metalliferous character, portions being apparently rich, while others are completely barren. With the present knowledge upon the subject, we can scarcely arrive at safe conclusions as to the value of these veins for the purposes of mining, but upon the whole they may be looked upon favorably rather than otherwise.

Adjourned to 4 o'clock this afternoon.

Friday, 4 o'clock, P. M.—*Prof. Silliman* being absent, *Dr. Locke* was called to the chair.

Dr. Jackson gave his views in relation to the construction of geological maps, suggesting the importance of concert and uniformity in design and execution, as regards *scale*, *coloring*, *symbols*, &c. on the part of the various state geologists employed throughout the Union. The subject was further discussed by Dr. Locke and Prof. Johnson, who concurred in the views of Dr. Jackson.

On motion of *Prof. Mather*, the subject was referred to a committee consisting of *Dr. Jackson*, *Dr. Locke*, and *Prof. Mather*, who are to report at the next annual session of the Association.

Prof. Johnson exhibited a section drawn across the Frostburg coal basin, extending between the Little Alleghany and Savage Mountains, a distance of about four miles. He offered some observations concerning this coal-field, and enlarged upon its value and importance as a coal and iron region.

Mr. Hodge and *Mr. Trego*, who had explored that portion of this basin which extends into Pennsylvania, also made some remarks in which they differed from the views of Prof. Johnson; particularly with regard to his opinion that some of the upper strata of the carboniferous rocks near the Savage Mountain, rest *unconformably* upon the lower ones.

Mr. Hodge placed on the table some clay concretions from Kennebec river.

On motion of *Dr. Beck*, *Resolved*, That when this Association terminates its present session, it adjourn to meet in Boston, on the last Monday in April next.

Dr. Jackson, *Prof. Hitchcock*, and *Mr. Moses B. Williams*, were appointed a local committee, (pursuant to a resolution re-

ported by the committee on business,) for the purpose of making suitable arrangements for the next session of the Association.

The secretaries were intrusted with the preparation and publication of an abstract of the proceedings of the Association.

Resolved, That the chairman of the present session be requested to open the next session by an address.

Dr. S. G. Morton was then appointed chairman, and *Dr. Jackson* secretary, for the next session.

Dr. L. C. Beck, *Prof. H. D. Rogers*, *Prof. Hitchcock*, *Dr. Locke*, and *Dr. Jackson*, were appointed a committee to prepare a constitution, by-laws, &c. for the government of the Association, according to a resolution of the committee on business.

Dr. Griscom made a communication respecting the Duane "steel ore" of New York.

Adjourned to 9 o'clock to-morrow morning.

At 8 o'clock in the evening the Association had the pleasure, in common with a respectable audience of ladies and gentlemen of Philadelphia, of hearing a most interesting and instructive address from *Prof. Silliman*, on the general principles of geology, and subjects connected with its progress in America.

Fifth day of session, *Saturday*, *April* 10, 1841. The Association met this morning according to adjournment of yesterday. *Dr. Locke* in the chair.

The minutes of yesterday's proceedings were read and adopted.

Dr. Morton opened before the Association a vessel of earthen ware taken from the Pyramids of Sakhara in Egypt, and forwarded to him by the American consul at Cairo, which contained an embalmed body of the *Ibis religiosa*, or sacred bird of the ancient Egyptians. The earthen vessel containing this relic of the most remote antiquity, was of a cylindrical or rather a conical shape, having a lid or cover fixed on the larger end, closely fitted on and luted with a composition resembling common mortar of lime and sand.

Dr. M. remarked upon the interest attending these relics, owing to their complete preservation—the bones, feathers, and even animal matter being frequently found almost unchanged, except by desiccation. The specimen opened was one of uncommon interest on account of the great perfection and almost interminable number of the bandages of linen cloth in which it was enfolded, and the high preservation of the most delicate parts of the plumage. The

position of the bird in the embalmed specimens is found to be invariably the same. The neck and head are drawn down between the legs, the lower mandible being presented outward and downward, and the legs drawn forward beneath the body of the bird, as if in a sitting posture, with the wings folded over the neck and legs. Some specimens less perfectly bandaged seem to have undergone a process of carbonization, and on the removal of the linen folds crumble into a dark powder, in which the bones appear, though reduced to a brittle state.

Dr. Morton referred to the very recent appearance of the pottery ware in which these specimens were contained; notwithstanding their extremely ancient date, which is at least three thousand to four thousand years. The pyramids of Sakhara are among the most ancient monuments of human art. These cases containing the embalmed ibis are still found in great numbers, though the traveller, Dr. Pococke, gave his opinion one hundred years ago, that they would probably soon become extinct.

Dr. Morton then proceeded to open another envelope containing some unknown embalmed object, which he conjectured to be a mass of snakes or serpents. This was less carefully enclosed than the ibis, being coarsely enveloped in rags rather than bandages, though still covered by hundreds of folds of linen. These being at length removed, disclosed *the wing of a bat!*

The next embalmed object unfolded was a young crocodile, (*Crocodilus niloticus,*) about a foot in length, and in good preservation. Dr. M. observed that this animal is found in embalmed specimens of all sizes, from the apparently just hatched young to those of five feet in length, one of the latter size being at present in his collection.

Mr. Quinby exhibited specimens of silver, lead, and other ores from the Andes in Peru, accompanying them with some observations upon their product, situation, &c.

Prof. Johnson showed specimens of magnetic iron ore from the State of New York, which he had found to contain titanic acid, combined with iron and manganese.

Dr. Locke made some observations on the application of magnetism to the discovery of metallic veins and deposits.

The following resolutions being moved by *Prof. Rogers*, were unanimously adopted.

Resolved, 1. That the thanks of the Association be presented to *Prof. Silliman*, for the interesting lecture delivered by him last evening.

2. That the thanks of the Association be presented to the Academy of Natural Sciences for the use of their rooms during the present session.

3. That the sincere thanks of the Association be presented to *Prof. Silliman*, for the highly able manner in which he has discharged the duties of chairman at the present annual meeting.

4. That the thanks of the Association be presented to *Dr. Beck* for the able and laborious manner in which he has discharged the duties of secretary throughout the first year, and at the present meeting. Also that the thanks of the Association be given to *Messrs. B. Silliman, Jr.* and *Charles B. Trego*, for their valuable services as assistant secretaries.

On motion of *Prof. Rogers*, amended by *B. Silliman, Jr.*, it was *Resolved*, That the Association publish five hundred copies of the address of Prof. Hitchcock, under the direction of the secretaries, which shall be distributed, as soon as practicable, to all the members of the Association; and that the expense of publication be defrayed by a pro rata charge on each member, to be paid at the next meeting of the Association in Boston. Such copies as are not distributed to members under this resolution, to be sold for the benefit of the Association.

On motion of *Prof. J. C. Booth*, it was also *Resolved*, That the names of all the officers of the Association, of the local committee, and the names and addresses of all the members of the Association, be appended to the address of Prof. Hitchcock.

The Association then adjourned to meet in Boston on the last Monday in April next, (1842.)

B. Silliman, *Chairman.*

L. C. Beck, *Secretary.*

B. Silliman, Jr. } *Assistant Secretaries.*
Charles B. Trego, }

II. *Boston Society of Natural History.*

At a regular meeting of the Society, held on the 21st day of April, 1841,

Dr. C. T. Jackson having announced that at the late meeting in Philadelphia of the Association of American geologists, it had

been determined to hold the next meeting of that Association in this city, in the month of April, 1842, it was

Voted, That this Society invite the Association of American geologists to make use of the hall of this Society for the meetings of that Association, contemplated to be held in this city in April, 1842, and tender the use of the cabinet and library for the purposes of the Association. A true copy of record. Attest,

T. Bulfinch, *Sec. pro. tem.*

MISCELLANIES.

FOREIGN AND DOMESTIC.

1. *Proceedings of the Geological Society, June 10th*, 1840.—A paper was read on the polished and striated surfaces of the rocks which form the beds of glaciers in the Alps, by Prof. Agassiz.

This paper was accompanied by a series of plates, intended to represent the effect of glaciers upon the rocks over which they move.

These effects, consisting of surfaces highly polished, and covered with scratches, either in straight lines, or curvilinear, according to the direction of the movement of the glacier, are constantly found, not only at the lower extremity, where they are exposed by the melting of the glaciers, but also, whenever the subjacent rock is examined, by descending through deep crevices in the ice. Grains of quartz, and other fragments of fallen rocks, which compose the *moraines* that accompany the glaciers, have afforded the material which, moved by the action of the ice, has produced the polish and scratches on the sides and bottom of the Alpine valleys, through which the glaciers are constantly but slowly descending. It is impossible to attribute these effects to causes anterior to the formation of the glacier, as they are constantly present and parallel to the direction of the movement of the ice. They cannot be considered as the effects of an avalanche, for they are often at right angles to the direction in which an avalanche would descend; they are constantly sharp and fresh beneath existing glaciers, but less distinct on surfaces which have for some time been exposed to atmospheric action by the melting of the ice. In the valley of the Viesch, the direction of the scratches is from north to south, or towards the Rhone; the direction of those which accompany the glacier of the Rhone is from east to west; that of those beneath the glacier of the Aar is first from west to east as far as the Hospice of the Grimsel; and then from east to north, from the Grimsel to the Handeck. If we could account for these scratches by the action of the water, we must im-

agine currents of enormous depth filling these highest Apine valleys, and descending in opposite directions from the narrow crest that lies between them. In the upper part of the valley of the Viesch, is a glacier, beneath which runs a rapid torrent, coextensive in length with the great current to which the above hypothesis would attribute the polish and scratches on the rocks of the valley.

This small torrent corrodes the bottom of the valley into sinuous furrows and irregular holes, and polishes the sides of its bed; but the polish is of a different aspect from that produced by the action of the ice, and of the stones and sand which it carries with it. The polished surfaces beneath the ice are often salient and in high relief. The sides also of the valleys adjacent to the actual glaciers, are frequently polished and scratched at great heights above the ice, in a manner identical with the surface beneath it, but different from the polish of the bed of the torrent.

The amount of polish and scratches varies with the nature of the rocks. In the valley of Zermalt and Riffelhorn, rocks of serpentine are most exquisitely polished; so also are the granites on the sides of the glacier of the Aar, where they have not been long exposed to the action of the air. Gneiss and limestone do not preserve their polish under similar exposure, but retain it while they are protected by ice or a covering of earth.

These facts seem to show that the striated and polished condition of rocks beneath and on the sides of glaciers, is due to the action of the ice, and of the sand and fragments of stone forming the *moraines* which accompany it.—*Extracted from the Annals and Magazine of Natural History, for January*, 1841.

2. *The former existence of Glaciers in Scotland.*—The late visit of M. Agassiz to Scotland, during the meeting of the British Association, seems to have set all our geologists off upon a new scent,—glacier hunting. That distinguished zoologist and geologist, by his interesting work and illustrations on the glaciers of Switzerland,* has, we think, proved that they formerly existed at a much lower level than they do now on the Alps of the continent, and anxiety to examine a country where glaciers no longer existed, was the immediate motive of his visit to Scotland during the last autumn. In company with accomplished English and Scotch geologists, the examination was accordingly made, and the same appearances which characterize the rocks under the European glaciers being observed in various parts of the higher mountain ranges of Scotland, induced M. Agassiz to believe that they formerly existed in these mist-clad regions, and that many of the phenomena attributed to the action of water, such as the parallel roads of Glenroy, &c., were caused by their influence; and he writes thus on the subject to Prof. Jamieson:

* Etudes sur les Glaciers, par L. Agassiz, dessinées d'après Nature et Lithographies, par J. Bettannier, 1840, Neuchatel.

"After having obtained in Switzerland the most conclusive proofs, that at a former period the glaciers were of much greater extent than at the present; nay, that they had covered the whole country, and had transported the erratic blocks to the places where these are now found, it was my wish to examine a country where glaciers are no longer met with, but in which they might formerly have existed. I therefore directed my attention to Scotland, and had scarcely arrived in Glasgow, when I found remote traces of the action of glaciers; and the nearer I approached the high mountain chains, these became more distinct, until, at the foot of Ben Nevis, and in the principal valleys, I discovered the most distinct *moraines* and polished rocky surfaces, just as in the valleys of the Swiss Alps, in the region of existing glaciers; so that the existence of glaciers in Scotland at early periods can no longer be doubted. The parallel roads of Glenroy, are intimately connected with this former occurrence of glaciers, and have been caused by a glacier from Ben Nevis. The phenomenon must have been precisely analogous to the glacier lakes of the Tyrol, and to the event that took place in the valley of Bagne."

At one of the early meetings of the Geological Society of London, M. Agassiz read a paper, illustrating his views and their application to Scotland. This was followed on the 4th of November by a long paper from Dr. Buckland, on the same subject, and which was concluded on the meeting of the 18th, but reserving its more minute details for a subsequent evening; while Mr. Lyell has also commenced the reading of a paper "On the Geological evidence of the former existence of glaciers in Forfarshire." Dr. Buckland, in his paper, gives a general account of his late tour in Scotland, and among the more remarkable parts of his communication is the announcement that the traces of ancient glaciers are apparent in Crickhope Linn, in Nithsdale, Dumfriesshire, upon the rocks of Stirling and Edinburgh castles, and upon Corstorphine, the Calton, and Law Hills, near Edinburgh. This theory, as applied to Scotland, is comparatively new; and in the animated discussions to which it has just given rise, has been combatted, or strictly scrutinized, by Messrs. Greenough, Sedgwick, Murchison, Whewell, Phillips, and De la Beche. We have no doubt that the traces, as stated by the Professor, exist in all the above named localities; but while such is the case, it becomes most important to ascertain if these appearances, at so low an elevation, *could have been produced by any other action than that of ice.—From Annals and Magazine of Natural History, for Jan.* 1841.—On this subject, see also a paper in the January number of the same Magazine, "*On the Natural Terraces of the Eildon Hills, being formed by the action of Ancient Glaciers*," by Mr. Bowman; from which we extract a single paragraph. "In conclusion, Prof. Agassiz informed me that he had traced repeated instances of the various descriptions of *moraines* in different parts of Scotland; also in Ireland, and between Sharp and Kendal, in Westmoreland;

and he does not doubt that they will be recognized, now that the attention of geologists is directed to the subject, in North Wales, in the Pyrenees, the Appenines, and other high mountain chains. Indeed, he believes, from strong evidences scattered over different countries, that at a recent geological period, and not long before the creation of the human race, the whole of Europe, and those parts of Asia and America which lie north of the parallel of the Mediterranean and Caspian seas, were enveloped in snow and ice; in short, consisted of a series of immense glaciers, above which, only the highest hills appeared as islands; presenting a character of scenery to be found in our day only in Greenland or Iceland."

3. *Red Color of the Salt Marshes of the Mediterranean.*—The red color of these marshes, often of a very deep tint, has been for a long time attributed to the presence of a minute crustaceous animal, *Artemia salina*, Leach. Mons. Joly has last year attended to this subject, and has come to the conclusion that the color is produced, not by the *Artemia*, but by minute animalcules, occurring in incalculable numbers, and to which he has given the name of *Monas dunalii*. This is fed upon by the Artemia, to which it communicates its brilliant red color, &c.—*From Annals and Magazine of Natural History, for Dec.* 1840.

4. *New observations on the Infusoria of Rock Salt.*—In the '*Comptes Rendus*,' (March 16,) mention is made of a note received by the Academy of Sciences by M. Marcel de Serres, relative to the observations which he is making on this subject along with M. Joly. In the specimens of rock salt of a tolerably decided greenish color, brought from Cardona, (Spain,) the infusoria appear more rare, smaller, and less distinct than in the specimens of a red color before examined. This, says M. Marcel de Serres, finds an explanation in M. Joly's previous observations on the changes of tint which the infusoria that color our salt marshes undergo by age. These animalcules, which are white at their birth, become green in their middle age, and do not till their adult age, take the purple tint which makes them so remarkable. In general, the green infusoria are not so often seen in salt marshes as the red, which seems to indicate that these monads remain but a short time in their middle state. We have found the same infusoria in the argillo-calcareous marls of Cardona beneath the rock salt. There they have their beautiful purple tint, but they are in too small numbers to communicate it to the mass of marl, which has remained grayish. This fact also proves, that in the ancient world, as in the present one, the animalcules were precipitated after their death to the bottom of the waters in which they previously lived.—*From the Annals and Magazine of Nat. Hist. for Sept.* 1840.

5. *Ornithological Gallery of the British Museum.*—The eastern gallery of the British Museum, which was formerly occupied by the collec-

tions of minerals, having undergone a complete repair, has been lately reopened to the public, with the collection of birds and shells. Only the passerines, gallinaceous, and wading birds are as yet arranged; but the remainder will be exhibited in the course of the spring, when this room, which is three hundred feet long and fifty wide, will contain one of the richest ornithological collections in Europe. The cases are all glazed with large panes of plate-glass, with very narrow brass bars; and the smaller birds are arranged on a new plan, on box shelves, each bird having a background close behind it, so as to show its outline distinctly and relieve its colors; and the shells, which will occupy forty table cases, are exhibited on black velvet, which gives them admirable relief.—*Ann. and Mag. of Nat. Hist. for Dec.* 1840.

6. *Prof. Agassiz and his Works.*—In former volumes of this Journal, we have mentioned the labors of Prof. Agassiz on the fossil fishes, the echinodermata, and the living fresh-water fishes of Central Europe.

1. His great work on Fossil Fishes. 2. Monograph on the Echinodermata, living and fossil. 3. On the fresh-water Fishes of Central Europe, Part I. And we now announce the reception from him of 4. His Description of the Fossil Echinodermata of Switzerland, Parts I and II. 5. Memoir on the moulds of Molluscs, both living and fossil, Part I on the moulds of the living Molluscs. 6. Critical Study of Fossil Molluscs, Part I, containing the Trigoniæ of the Jura and of the Chalk of Switzerland. 7. Systematic Catalogue of the Echinodermata. 8. Study of the Glaciers, 1 vol. 8vo. with a folio Atlas containing thirty two plates.

The Swiss formations prove to be unexpectedly rich in fossil echinodermata, hitherto undescribed, as appears from the extensive catalogue of those which M. Agassiz has collected. It is his intention (as announced to us in a letter dated Jan. 8, 1841) speedily to publish monographs of all these species as well as of those that are living. Two of these parts have already appeared, and a third part is ready prepared. In his critical study of the Molluscs, the author intends, in like manner, to describe the species of these genera that have hitherto been the most neglected.

The first monograph of the Trigonias, now before us, affords a striking example of the great amount of materials which the author has collected. In the memoirs upon the moulds of Molluscs, we may confidently expect that a new era will open upon conchology, by proving the possibility of determining, with rigorous exactness, the fossil moulds so frequent in certain geological formations, and which have hitherto remained undetermined.

The study of the glaciers will probably excite a more vivid interest than any of the works of Agassiz, especially since the discovery made by him, during a residence of some months in the British Islands, of marks indicating, that in former ages a very extensive shell or crust of ice extended

over all the chains of mountains in Scotland, Ireland, and England, a discovery which brings strong confirmation to his theory. The Athenæum, Literary Gazette, reports of the proceedings of the Geological Society, &c. have given a particular account of his observations, and of those of Dr. Buckland and Mr. Lyell on this subject. We have inserted a notice from the hand of a friend, but had not room in this number for a much fuller account in the reports of the Geological Society.

It will be very important that American geologists, guided by the work of Prof. Agassiz, should attentively study the similar phenomena which may doubtless be observed in North America. The works of the author abound with fine plates, and whether of minute molluscs or stupendous icy peaks, and domes or seas of ice, they are highly illustrative and beautiful. His works being very expensive, he has been repeatedly aided by funds from the British Association, but several of them have been undertaken at his own risk. Most of them are quartos, but the plates are in some instances folios. We invite American naturalists and institutions to encourage these arduous and responsible labors by purchasing the works, which may be obtained through Mr. Augustus Mayor, of New York, No. 63, William st.

The first livraison of the fossil echinodermata of Switzerland, costs fifteen francs; the second, twelve francs; the fossil molluscs, twelve francs; the moulds of molluscs, twelve francs; the glaciers with the splendid atlas, forty francs; the catalogue of the echinodermata, one franc. We are astonished at the variety, precision, and extent of the labors of this illustrious naturalist. He proves in his own case the possibility of cultivating successfully, at the same time, several different divisions of a great subject, and of bringing them out with almost unprecedented rapidity, and still without confusion or inconvenient delay. He has already pledged himself to so many important sequences of his begun labors, that we feel equally obligated and disposed cordially to wish him health and long life.

7. *Sketch of the Geology of North America, being the substance of a memoir read before the Ashmolean Society, Nov.* 26, 1838; by Charles Daubeny, M. D., F. R. S., &c. Oxford, 1839.—Although some time has now elapsed since the reading and publication of this memoir and since its reception by us, yet as it contains matter of considerable interest, it may not be amiss (although this notice has been longer delayed than we could have wished) briefly to review its contents, especially as it comes to us from one to whom science is under very many obligations.

From the short time which the writer passed in this country, having been absent from home scarcely a year, including a visit to Cuba, it cannot be expected that personal observation alone would have enabled him to give even a general view of the geology of our country, and we accord-

ingly find, that he acknowledges his indebtedness to naturalists here, and more particularly to the reports which have been made upon the geology of the states, for many of the facts presented.

After some preliminary remarks in relation to the space occupied by the country, the advantages to be derived from the geological surveys now making of the several states, &c. the subject of diluvial action or rather of the indication of the action of water upon the continent is taken up, and the phenomena ascribable to this cause, brought to light by the observations of Prof. Hitchcock, Dr. Jackson, and others, are dwelt upon at some length. Reference is also made to an article in the first volume of the Transactions of the Literary and Historical Society of Quebec, by Major Bonnycastle, and extracts are given, from which we learn that the same evidence is presented by an examination of the country north of the United States, as has been furnished from within our borders and elsewhere, of the violent action of running water or moving ice, or of that agent, whatever it was, by which the large masses of rock were moved, and by which the dry land has been generally strewn over with bowlders and debris.

The view presented by Mr. Hayes, a few years since, in an article upon the geology of western New York, that the action of the tides and waves alone upon the rocks subsequent to their elevation above the ocean, would account for all the effects usually attributed to the action of running water, does not meet the favor of our author. He thinks that Mr. H. in adopting it, "overlooked the fact, that the force of submarine currents extends but a little way beneath the surface of the ocean, and that even the Gulf Stream could have no power to move a block of stone along the bed of the sea underneath it so soon as it was once safely deposited upon its bottom."

From the consideration of the changes caused upon the surface of the country by the effects of water, the memoir next refers to those that have resulted from fire, and which by subterraneous action uplifted from beneath the waves, the great chains of the Alleghany and Rocky mountains, and gave rise to the thermal springs associated with them. In this connection is presented an account of the unstratified primary rocks. Commencing with those of the Blue Ridge of the south, they are described as extending north and east to New York, and into New England, where they are the predominating rocks. The two groups of mountains composed of these rocks, east and west of Lake Champlain, are mentioned as uniting in Canada, and there blending in a low chain which crosses the continent northwesterly, the primary strata being visible until approaching Lake Winnipeg, where they are lost sight of beneath the cretaceous group. This class of formations is also referred to as prevailing in the Rocky mountains, travellers there, wherever observations have been made, having noticed granite, gneiss, quartz rock, &c.

In a wide sense then, says the memoir, it may be stated that the central portion of the continent is surrounded on the north, east and west, by a chain of primary formations, and that the entire space included betwixt the Alleghany and Rocky mountains, constitutes the great valley of the Mississippi. In so terming the immense country intermediate between these two ranges of mountains, the reader is guarded against the idea, that this tract is filled up with alluvial or tertiary deposits, or of supposing that it maintains an uniformly low level. This, as stated, can only be said of that portion situated between the Ozark and other hills, and the western side of the Alleghanies, about one hundred miles east and west of the Mississippi.

The Ozark hills, Dr. Daubeny, who had an opportunity of examining them in person, describes as consisting of clay slate, quartz rock, and sandstone, except near the mining district of Missouri, where porphyries and specular iron, in immense masses, have been protruded.

The several ores of the unstratified rocks are noticed, as are also the other minerals with which they abound. The dykes so common in New England, are mentioned as establishing a distinction between the rocks on the east, and those on the west of the primary chain, and as furnishing evidence of frequent igneous action subsequently to the elevation of the Alleghany mountains.

The memoir next proceeds to describe some of the later formations; i. e. the fossiliferous rocks of New York, and those of the same relative position in the more southern states, with their mineral and paleontological contents. This, although mostly drawn from the able reports of the gentlemen engaged in the surveys of these formations, is yet interspersed with much interesting matter, drawn from the observation of the writer. Of this character are the remarks upon the Falls of Niagara, which are well worth the careful perusal of all who feel at all desirous of information in relation to their origin. The difference of level between the country below and that above Queenstown, has, it is well known, been generally ascribed to a subsidence of the former. Dr. Daubeny, however, from noticing that the strata from this place to Lake Erie appeared to dip somewhat south, (a fact, which, as stated in a note annexed, has been since verified by Mr. Hall,) is inclined to the opinion that this portion has been elevated. In relation to the falls, in other than a geological view, Dr. Daubeny, expresses himself as having been at first somewhat disappointed, though subsequently this feeling gave way to others of a different character. His own account is as follows:

"With respect to the fall itself, considered in a picturesque point of view, or as influencing the imagination and feelings, it may seem an odd confession to make, that my first feelings, on visiting it, were not unmixed with disappointment. Of an object so long known by report, each person before he reaches the spot conjures up in his mind some sort of idea,

which in certain respects must differ from the reality, and may therefore lead him to imagine the latter has fallen short of his previous conceptions. Thus I had imagined that the fury of the waters after they had been launched over the cataract would have been more terrific, and was surprised at seeing the ease with which an insignificant ferry-boat crossed the stream within a very short distance below. The noise produced by the waterfall itself, I had also conceived would have been more stunning; and it was with a feeling nearly allied to what one might entertain at hearing a person of solid weight and character, talked down by a noisy upstart of yesterday, that I found the roar of this stupendous natural phenomenon overpowered by the hissing of a locomotive which was letting off its steam at the railroad station adjoining.

"The presence of these evidences of human ingenuity was in other respects also very unpropitious to the feelings which the scene itself was calculated to inspire; and though no enemy to railroads or manufactories in their proper place, I could have wished all vestiges, both of the one and of the other, banished from a spot where nature ought to be allowed to reign undisturbed and alone.

"But after a time, these first prepossessions wore away, and I then began to feel more impressed with the solemnity of the sound which the cataract produces in its descent, than I had expected to have been, by the deafening tumult of waters for which my imagination had prepared me.

"In surveying it too under various aspects and at different distances, I found new sources of admiration and astonishment continually presenting themselves, of which I had previously no conception; nor did the interest of the scene appear to flag, when I turned to contemplate the phenomena presented in the course of the river both above and below, which may be regarded, either as concomitants, or as consequences of the cataract itself."

This is followed by a graphic account of the various features exhibited by the majestic waterfall at the different points from which it may be observed, as also of the scenery connected with it.

In relation to the sandstone of the Connecticut river, Dr. Daubeny in giving Prof. Hitchcock's opinion, that it is equivalent to the new red sandstone of England, merely remarks, that if this be true, it is singular that no salt is found within it, this being its most appropriate position. He visited one of the localities of bird tracks, and compared the specimens on the surface of the rocks with those in the cabinet of Prof. Hitchcock, and fully satisfied himself that they could have been produced only by "birds of various sizes treading upon soft and plastic material."

An account is given of the coal fields and brine springs of the United States, as also of the thermal waters; the last being treated quite at length.

In conclusion, it may not be amiss to state, that the whole is written in the same easy and agreeable style as the other works of its distinguished author.

8. *Outlines of Anatomy and Physiology, translated from the French of H. Milne Edwards, M. D. &c., by J. F. W. Lane, M. D.* Boston, Charles C. Little and James Brown, 1841. pp. 312.—This treatise was intended as an introduction to a work on zoology by the same author. It gives us a general outline of the anatomical structure and physiological action of the different systems of organs of which animal bodies are composed; also those general characters and properties of living beings which could not with propriety be introduced into the body of a zoological treatise. It is written in a popular style, and is sufficiently free from technicalities to render it interesting and intelligible to a non-professional reader.

Before entering into a description of the individual functions and their organs, those properties are enumerated which are common to all living beings, and by which they are distinguished from common inert matter, viz. their mode of origin, structure and chemical composition, powers of nutrition, reproduction, and the definite duration of their existence; also the characteristics by which animals are distinguished from vegetables, the former having, in addition to the properties possessed by the latter, sensibility and voluntary motion.

The different functions, and the organs by which they are performed, are arranged in three great divisions, viz. those of nutrition, relation, and reproduction. Under the first head, are described the blood, its properties, the apparatus and mechanism of its circulation, exhalation, secretion, respiration, animal heat, and digestion: under the second head, are arranged those organs and functions by which animals are made acquainted with, and are enabled to act upon external objects, viz. the sensorial, intellectual, motory, and vocal; and under the third head, those which are subservient to the preservation of the species, viz. those of generation.

In treating of the different functions first enumerated, it is the object of the author to describe them as they exist in man, and compare with them those of which the lower orders of animals are the seat. In so doing, he has placed the subject of physiology under a very attractive form; and we trust that the work will be extensively circulated, and that it may conduce still farther to develop that taste for the science of which it treats, among the unprofessional public. The translation of the work is in some respects objectionable, the technical words being, in many cases, literally rendered, instead of substituting for them the corresponding English technical expressions. We have no hesitation, however, in saying, that the high merit and reputation of Dr. Edwards as a physiologist and zoologist, and the interest which is felt by the public in anatomical and phys-

iological sciences, will render the work, at the present time, highly acceptable.

9. *Volcanic Phenomena in Hawaii.*

To Prof. B. Silliman—*My dear Sir:* You expressed a wish that I might send you an extract from the letter of Capt. Couthouy; and as my way of doing business is never to delay, if avoidable, I send it you. The letter is dated Honolulu, Oahu, Oct. 24, 1840.

"Visited the great crater at Kirauea or Ka Lua Pele, which is an immense pit one thousand feet deep and six miles in circuit, with perpendicular walls, except at one point, where it is reached by a steep descent, and the whole of this vast cauldron, full of boiling, bubbling, and spouting lava. The surface at one moment black as ink, and the next exhibiting rivers and pools and jets of a hideous blood-red fluid, that was sometimes thrown up to a height of fifty or sixty feet, and fell back with a sullen plashing that was indescribably awful. The aspect of the whole was *hellish*—no other term can express it. By night it was grand beyond description. The frequent lightings up, the hissings and deep muttering explosions, reminded me of some great city in flames, where there were magazines of gunpowder or mines continually exploding. Vesuvius is a fool to it. Just previous to my visit, the lava had burst out at a new place, about six miles northeast of the crater, and flowed down to the sea in a stream of forty miles in length, by from one to seven in breadth. I saw the light one hundred miles off. It reached the sea in five days, threw up three hills (I send a rude sketch, but literally correct, and interesting as the work of a native) of from one hundred and twenty to two hundred and fifty feet high, gained two thousand feet out seaward from old line of coast, by three fourths of a mile in width, and heated the water for fifteen miles either side, to such an extent, that the fishes were heaped up in myriads on the shore, scalded to death. Its falling into the sea, was accompanied with tremendous hissings, and detonations like constant discharges of heavy artillery distinctly heard at Hilo, twenty minutes distant."

With great respect, yours, truly, D. H. Storer.

10. *Manilla Hemp.*

Singapore, May 29, 1840.

To Prof. B. Silliman—*Dear Sir:* Having had my attention called to the subject of Manilla hemp, and the mode in which it is prepared, I have had the good fortune to receive from a friend on the spot such an account of the matter as may be interesting to some of your readers. My correspondent is T. M. M., Esq., a British merchant in Manilla. His paper is chiefly a translation of Blanco's Flora Filipina, from page 247 to 250. He has enjoyed the best opportunities for obtaining information on

the subject, and has freely made his remarks as the various topics in the translation gave occasion for them. He has sent me a specimen of some of the hemp which he made himself, while inspecting the operation; a portion of which I send you. As a part of his paper is taken up in controverting some of the opinions of Blanco, I shall omit this, and give that part of the process on his authority, he speaking from personal observation, and Blanco from report. Having obtained his consent, you are at liberty to make such use of the paper as you judge most suitable.

Yours, &c. Jos. S. Travelli.

This article is obtained from a species of plantain, the "Musa Troglodítarum textoria," and is probably a variety of the Musa Troglodítarum errans; the lower edge almost without incision, stamens five, without rudiments of sex, fruit with three ribs and many perfect seeds. It is one of the most useful plants and is cultivated with care in the province of Camarines, and in other parts. At first sight it is not different from others of the same species. The fruit is very small, seldom exceeding two inches in length. The use made of this plantain is immense; of it are made ropes, cables, and woven cloth of extreme fineness.*

In preparing the hemp they cut the tree near the ground at the time it is about to produce fruit, and also cut off the upper extremity or head, removing the leaves. They tear off one by one the layers, and with a knife remove the skin from the inner surface. The layer or roll being now stript of its inner skin, is torn into strips of about two fingers' breadth. These strips are placed, one at a time, on a table or piece of wood, below the edge of a knife, which is pressed down upon the table by a long bamboo fastened in the earth.

1, long bamboo pressing the handle of the knife *upwards* to keep its edge closely down. 2, knife—perhaps somewhat like a small cleaver or butcher's chopping-knife. 3, pivot or support for the knife. 4, the table, or stake of wood or bamboo on which the strip is placed under the edge of the knife.

The strip of the layer being placed beneath the knife, the outer skin or surface uppermost, they pull it with force by one end. The knife re-

* The quantity exported annually from Manilla to Europe and the United States, is about 80,000 piculs, (i. e. of 140 lbs. each,) making about 11,200,000 lbs. English; of which, about four-fifths is said to go to the United States.

moves the skin, and the pressure on the table enables the table to remove the fleshy substance. It is usually pulled but once; that is to say, when the strip is placed under the knife; about two feet remain on the side of the operator, which he grasps to pull it through; this done, he turns it, placing the other end under the knife, and grasping the end which now shows only the fibre, he by the second pull finishes this part of the process. The handful of threads or fibres is then placed with others on a branch of a tree, or peg, in the side of his hut, till a quantity is accumulated. Hung up in this way for an hour or two, the fibres are dried and fit for merchandise. T. M. M. thinks that very little, at the most not a fourth of the fibre, is lost by this process. Blanco imagines one half, he also thinks it undergoes a second process, being passed under a sort of saw. T. M. M. supposes this to be only a bad way of performing the process just described, it being much easier to draw the strip under a saw than under a knife. But as a much larger portion of the skin is left adhering to the fibres in this way, it is much less valuable. The process by the knife is so tedious, that three men will make only thirteen and a half pounds in an ordinary day's work.

The fibres thus prepared are separated into various qualities by the women. They weave them with great dexterity, even in the dust. If the hemp is to be used for making cloth, it is first wound into a ball about the size of a child's head, which is then placed in the wooden mortar used for husking rice, and is pounded for some time with the wooden pestle. This operation renders it very flexible, and less liable to break. This being done, the fibres are tied together, which is usually done by women and girls. The weaving process is the same as for cotton fabrics; but if the hemp (i. e. plantain) fibres are very fine, they place the women who weave it under a cover or shade, because the wind easily breaks the threads. The cloth being woven, is placed for a day and night in water, with a little lime made from sea-shells. It is afterwards washed and stretched out. Although the hemp is strong, yet it is inferior to linen thread, and to thread of European hemp, both in respect of strength and in the gloss and touch. Hemp (i. e. of the plantain) is produced of good quality in the province of Batangas, and in other parts, but not equal in quality to that of Camarines province; and this latter again appears inferior to that of Panai and Marinduque Islands. On this point, however, opinions differ.

The fruit of the hemp plant is eaten. The water (sap or juice) collected in a hole cut in the trunk of the tree, at the foot, is said to be a remedy for contraction of the *membrum virile*, a singular complaint, which ceases not to be frequent in the Bisayas provinces, and which is generally accompanied by a contraction of the tongue. The Englishman, Dampier, was mistaken when he said, that the hemp plant was known only in the island of Mindanao.

The hemp is easily dyed of blue and pink colors. To dye it blue, they still employ, as in the olden times, the leaves of a shrub or twining plant, which, in Camarines, is named *payanguit* and *aringuit*, the *Marsdenia akar*. (*Flora Filipina*, *p.* 118.) An intelligent description of this twining plant was made to the Politico-Economical Society of Manilla, by that curious and diligent observer, Padre Jose de Mata, of the order of San Francisco; and it is he, who, in these our days, has brought this plant to the notice of the European dwellers in the Philipines. The leaves of this plant give blue color in abundance.

To dye the hemp pink, I have heard that in Camarines province they boil the bark of the root of the Morinda (*Flora Filipina*, *p.* 148) with a little lime or alum, till the desired color is obtained, and with this they proceed to dye. But it is better to dye in the same manner as used for cotton thread, which is with a solution of wood-ashes and oil of *Sesamum*. (*Flora Filipina*, *p.* 507.) Additional note by T. M. M., 30 Mar. 1840.

The hemp of commerce is brought to Manilla chiefly in native rigged boats of fifty to one hundred tons burthen, termed *pontines*, from Camarines and Albay provinces, and from Zebu Island. The price paid to the actual producer in the interior of the country, I do not know; but considering that it is a bulky article, the cost of transportation must be heavy, as it is brought to the ports of shipment for Manilla, chiefly on horseback, through a country where there are few roads, and must also be collected from house to house; the price, therefore, that can be afforded to the native laborer must be very trifling. At the ports of shipment, when roughly made up into bales, it costs generally about —— per picul of 140 English pounds, on board the pontines. The general price at which they sell it in Manilla to the exporters is from $3,75 to $4,25 per picul. It is delivered from the pontine, at some of the various hemp screws, where it is opened out, examined, and again made up into well-shaped bales, each weighing two piculs, or 280 English pounds, net, and measuring about ten and a half cubic feet. In this state it is ready for transmission on board ship, and is generally known in commerce. The expenses of screwing, shipping, and two per cent. export duty, amount to about half a dollar per picul of 140 pounds.

11. *National Institution at Washington for the promotion of Science. Organized, May* 15, 1840.—Constitution adopted and objects declared, being to promote science and the useful arts, and to establish a National Museum of Natural History, &c. First list of officers.

Directors—Hon. Joel R. Poinsett, late Sec. of War, Hon. James K. Paulding, late Sec. of the Navy.

Councillors—Hon. John Q. Adams, Col. J. J. Abert, Col. Jos. Totten, Dr. Alex. McWilliams, A. O. Dayton.

Treasurer—William Stone.

Secretaries—Francis Markoe, Jr., Cor. Secretary, Pishey Thompson, Rec. Secretary.

A very able discourse was delivered before the Institution by the Hon. Joel R. Poinsett, at the first anniversary.

He states that the Institution "embraces every branch of knowledge." It is divided under the following sections, namely: Astronomy, Geography and Natural Philosophy, Natural History, Geology and Mineralogy, Chemistry, the application of Science to the Useful Arts, Agriculture, American History and Antiquities, and Literature and the Fine Arts. Gratuitous lectures to be given by eminent men. Collections of objects of natural history and of documentary information to be made from all quarters of this country, and of specimens from all foreign countries. Efforts to be made to rear a school of mines, which would be of great importance.

The ordnance officers lately returned from Europe have brought with them many specimens of iron ores used in foreign foundries, and those of the United States will be obtained and brought into comparison. The South Polar Exploring Expedition have already sent home one hundred and fifty boxes of specimens, which are stored away until a proper building can be provided to receive them.

Mr. Alexander Maclure, of New Harmony, Indiana, has offered a suite of geological specimens from the cabinet of his late brother, William Maclure, the pioneer of American geology, the munificent patron of the Academy of Natural Sciences at Philadelphia, and of many other institutions, recently deceased in Mexico.

The Institution has issued its first bulletin of proceedings, from June to December. Several of the sections are organized, being filled with able men. (We hope they will prove *workers*, as no machine will go without impulse.) Many donations are announced of minerals, books, documents, maps, curiosities, works of art, &c.;—and examinations of, and reports on various scientific and practical subjects are suggested and projected.

The bulletin contains a letter from the venerable Mr. Duponceau, on the prospects and interests of the society; it is exceedingly wise and kind, and its suggestions should be kept constantly in view.

There are instructive letters also from other eminent individuals at home and abroad—copious extracts from the admirable discourse of Mr. Poinsett, which shows enlarged views and extended knowledge, with the best spirit; also extended communications from Captain George W. Hughes, of the corps of topographical engineers, containing very instructive and important communications respecting the mines and metallic works and manufactures of South Wales, Cornwall and Devon, and many other important subjects.

Captain Hughes communicated also, as appears in the bulletin, a spirited sketch of the meeting of the British Association which he attended at Glasgow, in the autumn of 1840, with the views of many of its great men in scientific and practical geology.

On the whole, we are very favorably impressed by the plan and first inception of the National Institution at Washington, and we cordially wish it success. We trust that the government will respond to, and fulfill their high obligations in regard to the munificent half million bequest of Mr. Smithson, and will bring it to bear either in aid of this Institution, or in some other form, *without delay*, as neglect and procrastination which would be as little to our honor as to our advanage.

The frequent changes among our public men connected with the general government, creates no small anxiety as to the fate of any efforts to promote good knowledge and liberal arts, so far as a local institution at Washington may depend upon those who are oppressed with political duties, and whose action is so liable to receive a disastrous bias from the malign atmosphere of party. Liberal institutions at Washington, under the influence of enlightened and truly patriotic minds, could not fail to exalt the character, both of the government and of the country—of every such institution we would devoutly say, *esto perpetua!*

12. *Fossil Turtle.*—Dr. Mantell has discovered a small fossil turtle in the chalk of Kent—a very beautiful thing, and he has prepared a memoir on it for the Royal Society.

13. *Fossil Saurians.*—Dr. Mantell has communicated to the Royal Society an elaborate memoir upon the iguanodon, hylæosaurus, and other reptiles of Tilgate Forest, embodying all our knowledge of the osteology of those saurians, derived chiefly from his own researches.

14. *Microscopical Observations and Microscopes.*—Dr. Mantell, under date of London, March 29, 1841, writes to the senior editor of this Journal:

"Microscopical observations are now all the rage in every department of science. If you go to the Royal Society, you are sure of the microscopical investigation of the embryo—at the Medico-Chirurgical, microscopical observations on the blood corpuscules—at the Geological, microscopical observations on fossil teeth, and at the Linnæan, microscopical observations on vegetable organization. At the soiree of the president of the Royal Society, (the Marquis of Northampton,) microscopes are the great subject of attention, and then we have a microscopical society and a microscopical Journal.

"My young son has been very busy with his microscope in examining some American water from West Point, sent by Dr. Bailey, and we have seen some of your infusoria alive and as active as if they had not had a

sea voyage, and had not been bottled up for weeks. Prince Albert was at Lord Northampton's soiree on Saturday, and I showed him many fossil teeth and bones and infusoria with the microscope. Prof. Owen, who has been following up his odontography, that is, microscopical examination of fossil teeth, has discovered a remarkable structure in the large conical teeth of Jaeggar's Mastodonsaurus—the calcigerous tubes—the medullary or pulp canals, and the dentine are distributed in the most intricate and labyrinthine manner you can conceive; and in consequence, Mr. Owen has named the animal the Labyrinthodon. A part of the skull has been found in Warwickshire, in the red sandstone, and although but a fragment, presents anatomical characters which prove the original to have belonged to the Batrachian family, and not to the Saurians. He believes that it is to some reptile of this kind that the impressions (the so called Chirotherium) on the red sandstone, are to be attributed. The sections of fossil teeth are made in the same manner as those of fossil vegetables, as figured and described by Mr. Witham; and they present the most beautiful objects imaginable; but you will doubtless see Mr. Owen's book, in which they are admirably represented. Mr. Owen's memoirs, whether he be right or wrong in ascribing the foot-marks of the red sandstone to the reptiles that Jaegger describes as Mastodonsaurus, are admirable essays on a most interesting department of palæontology. You will be delighted with his beautiful elucidation of the form and structure of the original animals from a new fragment of the upper jaw and a few teeth. He has, perhaps prematurely, given a restoration of the Labyrinthodon, representing it as a Batrachian, with a head like a crocodile.

"Ross and Powell are the two rival opticians whose microscopes are in demand. From ten to forty or fifty guineas is the price of the best."

Another correspondent* says, "I have one which, in its simple form, has cost me eighteen guineas, and will cost fifty to complete it. I learn that they are made much cheaper at Berlin and at Paris, where Mr. Donne has constructed a very good one for thirty-six francs."

The correspondent first named, adds: "Fossil infusoria in flint are now greatly in vogue, particularly Xanthidia, like those figured in Mantell's Wonders of Geology. You have them living in the water at West Point. But Ehrenberg's grand folio work has induced an active search after living animalcules, and my son has discovered in a stream near us, almost every form figured by Ehrenberg."

15. *Geological Drawings.—Schorf* has been painting in distemper representations of some of the plesiosauri and ichthyosauri in the British museum on a large scale, from six to eight feet long, for public lectures. The price is from forty shillings to three guineas each. A copy of the

* Mr. G. F. Richardson, curator of the British Museum, London.

Wealden reptiles would be five or six pounds—size nine feet by five or six. The hylæosaurus would be about three guineas. Even the Maidstone iguanodon might be drawn on the same scale.—*Dr. Mantell.*

16. *Glaciers, Moraines, &c.*—You will have seen that Agassiz's "Etudes des Glaciers" has excited some of our principal geologists and others to hunt after moraines, and they instantly found proofs of former glaciers, and of a cap of ice over the whole northern hemisphere, from the bowlders and scratches on the rocks and beds of gravel and mud, which erst were considered indubitable proofs of diluvial action. As a consequence, several memoirs on the former existence of glaciers in England and Scotland were read at the Geological Society, and gave rise to many animated discussions. This is now over, and Mr. Owen's footmarks and labyrinthine tracks are on the ascendant. But we are steadily advancing in knowledge. Although Agassiz's theory, to the extent to which he carries it, cannot be admitted, yet modified, it explains many hitherto obscure phenomena.—*Dr. Mantell.*

17. *British Association—Mr. Murchison—His Journey to the Ural Mountains—Opinions, &c.*—A letter from Roderick Impey Murchison, Esq. to the senior editor, dated Paris, April 4, 1841, announces "*on authority*, that the next meeting of the British Association will be at Plymouth, (England,) on the 29th of July, and not on the 12th as was first promulgated."

Mr. Murchison was at Paris, on his way to the Ural mountains in Russia, in which tour he was to be accompanied by his friend, M. E. De Verneuil, the companion of his journey in Northern Russia in the summer of 1840. Of the geological results of this journey, an interesting account is given in a printed memoir of Messrs. Murchison and Verneuil, which we have just received. Their journey, the present summer, is for the purpose of settling some important questions in Russian geology, with reference to a geological map of that country. The travellers hoped to reach St. Petersburgh the first of May, and to be in the Ural mountains or their flanks in the first week in June.

It appears from Mr. Murchison's last memoir alluded to above, that "the evidences in Russia are most complete as regards a triple descending fossiliferous order of carboniferous, old red or Devonian and Silurian." The Rhenish provinces and Germany furnished, it is true, to Mr. Murchison and Prof. Sedgwick, the proofs of a similar order, but Russia alone has, as yet, supplied the union of proof required, viz. the order of superposition and the collocation in the same bed of the *Ichthyolites* of Scotland with the *shells* of Devonshire.

Mr. Murchison expresses his conviction that North America may offer the fullest and most perfect sequence of paleozoic strata in the world. He

adds, "To your magnificent region I look with intense interest, and I live in the hope of being able to explore its paleozoic rocks. Already, however, your countrymen are preparing all the elements for the complete classification of the older deposits of America. I have for some time been gratified to observe the steps which Conrad, Vanuxem, Hall, and others have been taking, and very recently I was delighted to receive from Mr. Hall a suite of specimens which leave no doubt of the descending succession of old red sandstone or Devonian rocks into the Silurian types. I sent his section to be laid before the Geological Society, with a notice from myself. Mr. Featherstonhaugh had furnished me with carboniferous and Silurian fossils, but the suite sent by Mr. Hall is the first that combines evidences of the existence of the great intermediate formation. The American old red sandstone with fishes is absolutely undistinguishable in hand specimens from certain beds with coccosteus, holoptychias, &c. which I have collected in the Highlands of Scotland, and largely in Russia."

We are authorized by Mr. Murchison to hope that his projected visit to the United States may not be postponed beyond the spring of 1843. Another eminent English geologist will visit us in the current summer—Mr. Lyell, as we learn by a recent letter from himself, may be expected in this country in August, and he will remain until the next year, travelling and engaged in professional objects. All our geologists will unite in welcoming these distinguished men, and they will receive a cordial welcome from many other intelligent persons.

18. *Dr. Brown of Edinburgh on the Production of Silicon from Paracyanogen.*—Much excitement has prevailed of late among the scientific world of Edinburgh, owing to the discovery claimed by Dr. Samuel M. Brown, a young chemist of that place, that one element may be derived from another by causing atoms of the *same* element to combine with each other under peculiar conditions, giving rise to bodies dissimilar in all respects to the properties of the original matter, and corresponding in character with the attributes of some other elementary body. In other words, one element (as we at present consider them) may be transmuted into another, and he conceives that all matter may proceed from one simple elementary body, which by union with *itself* under different conditions, gives rise to other dissimilar bodies possessing characters which have, we think, been considered elementary. He can combine but cannot separate the atoms.

It is now some months since we were first informed, through a correspondent in Edinburgh who is well known in this country, of this novel, and, as it seemed to us, extravagant claim, and we should never have thought of bringing the subject before our readers, had we not within a few days, through the same kind hand, received a copy in proof of Dr. Brown's second paper, the title of which is at the head of this notice,

read by Dr. Christison before the Royal Society of Edinburgh, May 3, 1841. In this paper the author claims to have decomposed paracyanogen by heat in a glass tube away from contact with atmospheric air, and to have obtained as a result nitrogen, and a body possessing all the known properties of *silicic acid*, (*silex.*) He attempts to prove that the silex could not be derived from the tube or crucible in which the experiments were performed, both on account of the quantity produced, the difficulty of conceiving of any act of substitution by which the silicon could be brought out of the glass or porcelain, and the fact that as nearly as could be ascertained no loss of weight was suffered by the containing vessel.

Dr. Brown's paper is in great detail, occupying about twenty pages of the Transactions of the Royal Society. "Dr. Christison," says our correspondent, "who read the paper, (the author not being a member of the Society,) added that he had seen the experiments performed, had studied every objection, and had exhausted his ingenuity in endeavoring to detect sources of fallacy, but without success; if there is error, it is unknown to him. The processes, he said, are delicate and difficult, and he will not renounce his faith in Dr. Brown until opposite results are obtained by more than one chemist equally skillful in manipulation, and equally patient and truth-loving in spirit. The Society unanimously thanked the author, and admitted his paper into their Transactions."

The present communication consists of five parts. The first treats of the production of silicon from paracyanogen; the second of the formation of amorphous mixed compounds of silicon with copper, iron, and platinum, by the reaction of paracyanogen on these metals; the third of the quantity of nitrogen separated from paracyanogen when it is changed into nitrogen and silicon; the fourth contains processes for the preparation of amorphous, semi-crystalline, and crystalline disiliciurets of iron from the paracyanide of iron and the ferrocyanide of potassium; and the fifth gives an easy process for the preparation of silicic acid on any scale of operation, by the reaction of the ferrocyanide of potassium on the carbonate of potassa.

We hope in our next to find space to publish this extraordinary communication entire, and it will then be in the power of all interested to verify or annul the author's results.—B. S. Jr.

19. *Artesian Boring at Paris.*—Many years ago, near the Barriere de Grenelle, one of the highest points in Paris, a boring was begun to obtain water. It was discontinued after some years, and again resumed about seven years ago. The result has been successful. We have room only for the following facts, recently communicated by a friend in Paris. The water was at last obtained below the chalk, at the depth of nearly eighteen hundred feet. The torrent of water, about three cubic yards per minute, rises in a copious fountain in the grounds of an *abattoir*, (slaughter-house,)

and is very pure. The column rises from a source one third of a mile below ground, and it spouts thirty feet above the surface. The temperature at the bottom of the boring* was nearly 83° of Fahrenheit, (that of a hot summer's day, such as is rarely known there on the surface,) thus confirming fully the increase of heat in the interior of the earth, by the average generally observed in similar cases of about 1° for fifty feet of descent, which, at the same rate of increase, would give a fountain of boiling water at two miles from the surface—full ignition of rocks at ten miles, and fusion at two hundred miles; thus leaving a firm crust to preserve the good citizens of Paris from being disturbed by the fear of breaking through, or by the danger of the immediate outburst of the fire.

20. *The Theory of Horticulture: or an attempt to explain the principal operations of Gardening upon Physiological Principles;* by John Lindley, Ph. D. F. R. S., &c. &c. First American edition, with notes, &c., by A. J. Downing and A. Gray. New York, Wiley and Putnam. Boston, C. C. Little and Co. 1841.—Few authors, at least in the English language, have been equally successful with Dr. Lindley in rendering *real science* intelligible and attractive to the general reader. This work, "written in the hope of providing the intelligent gardener, and the scientific amateur, correctly, with the rationalia of the more important operations of horticulture," has supplied a very important desideratum, and is deemed indispensable to every gardener and amateur cultivator in Great Britain. We presume it will be equally prized in this country, where a taste for horticulture is so widely diffused, and where a work, which conveys this kind of information, is so greatly needed. This edition is very neatly printed in the 12mo. form, and is afforded at an extremely moderate price.

21. *Corresponding Magnetic Observations, by Prof. A. D. Bache of Philadelphia, and Prof. Lloyd of Dublin.*—It is with much pleasure that we republish from the Proceedings of the Royal Irish Academy, (for June 22, 1840,) the following account,† by Prof. Lloyd, of Dublin, of a series of simultaneous magnetic observations made in November, 1839, by Prof. Bache at Philadelphia, and Prof. Lloyd at Dublin, with a view to determine whether any deductions could be drawn from them for determining differences of longitude. The results are very satisfactory, inasmuch as they prove definitely that "no correspondence whatever exists between the smaller changes of declination at Dublin and at Philadelphia, and that the determination of differences of longitude by means of the magnet at such distances is impracticable."—Eds.

* Which is over eighteen inches wide at the top, and from seven to eight at the bottom, and lined with a metallic tube.

† This account has been slightly abridged, to accommodate it to the crowded state of our pages.—Eds.

It is well known that the magnetic declination, at a given place, is subject to frequent and irregular variations, and that corresponding changes occur, at the same instant of time, at very distant places. The first recognition of this remarkable phenomenon seems to have been made by Arago, while comparing the observations of declination made by himself, at Paris, in the year 1818, with the contemporaneous observations of M. Kupffer, at Casan. Not long after, the subject underwent a fuller investigation in the hands of Humboldt; and, in the year 1827, an extensive system of simultaneous observations was organized by that illustrious philosopher for the purpose of elucidating it. At length, in 1834, it was taken up by Gauss, and received a much greater development. Gauss discovered that the irregular changes of the declination were of *continual* occurrence; and that the synchronism, which had been previously observed only in the larger changes, extended to the minutest movements. In order to investigate the law of these synchronous changes, and the locality and other circumstances of the acting forces, Gauss arranged the extensive plan of simultaneous observations at short intervals, which has been already four years in operation, and in which almost every country in Europe has been represented by some one or more observers.

The rapidity with which these changes thus appeared to follow each other, held out the hope that they might be employed in determining differences of longitude; and it only remained to ascertain, for that purpose, whether variations so inconsiderable in magnitude corresponded at great distances. If such should prove to be the fact, it would be only necessary to project on a large scale the results of the observations made about the same absolute time at the two stations, and to compare the times of the corresponding maxima and minima. In the observations already referred to, the maxima and minima succeeded each other (as has been said) at intervals of about forty seconds, and the epoch of their occurrence was probably known to six or seven seconds. By shortening still further the interval of observation, it is manifest that this error may be much diminished. The corresponding error of the difference of longitude resulting from a single comparison (supposing the probable error of epoch to be the same at the two places) will be greater in the ratio of $\sqrt{2}$ to 1; but this error, owing to the multitude of the maxima and minima compared, must necessarily be greatly reduced in the final mean.

In order to put this question to the test on the largest scale, it was agreed between Mr. Bache and Mr. Lloyd, to make a series of corresponding observations in Philadelphia and in Dublin. Some difficulties occurred in concerting a plan, and Prof. Bache underwent, in one instance, the labor of an extensive series of observations, without any counterpart in Dublin. At length, however, it was agreed to observe during the week commencing the 11th of November, 1839; the observations being taken during two hours on each day—namely, from 12 to 1 P. M., and from 8 to 9 P. M., Greenwich mean time.

Prof. Bache's account of his observations is contained in the following extract of a letter which accompanied them, dated November 29, 1839.

"The place of observation is a room in one of the out buildings for the dwellings of the professors of the Girard College. As the materials used in the construction of the house must produce considerable local attraction, no absolute measures have been attempted. All movable magnetic substances were removed from the vicinity of the needle. A window near the needle was carefully closed by a shutter of wood, and by two curtains fastened to the window frame, and with an interval between them. There is no fire in the room; and a double door is between the observing room and an adjoining one where there is a fire.

"The instrument is one of Gauss's declination magnetometers, made by Mayerstein of Gottingen. The arrangement of it agrees exactly with that described in the *Resultate*, which has been followed as nearly as possible. The reading telescope is supported upon a small wooden shelf fastened to one of the side walls of the room; the scale is attached to a wooden frame before the shelf. One of the smallest divisions of the scale, which is divided by estimation in the observations to tenths, is 25″.975 nearly in value. The zero of the scale did not vary sensibly in position during the observations.

"The observations were made every eight seconds, an assistant striking two seconds before the time of each observation. The ticks of the half-seconds chronometer being distinctly audible, the observation was made at the fourth beat after the signal given by the assistant, and thus the time was independent of the minute accuracy of the signal. Checks were adopted, to prevent or detect large errors in giving the signal. The interval of eight seconds is very nearly one third of the time of oscillation of the magnet bar.

"The time was observed by a chronometer beating half seconds. This was compared, before and after each set of magnetic observations, with one, and after the morning of the 14th with two chronometers. One of these was carried from the Girard College to the city after each set of observations; but the others remained during the night at the Girard College, and were removed to the city after the morning series, to compare them with the stationary chronometer belonging to the High School Observatory, the rate of which was ascertained by observations of transit of the sun and stars on the 6th, 9th, 11th, 13th, and 16th of November. Girard College is about 1770 feet west, and 8050 north of the High School by the city map."

The Dublin observations were made in the Magnetical Observatory. The instrument employed is of the form described some time since to the Academy.* It is a magnetic collimator with a graduated scale of glass,

* See Proceedings, No. 18, p. 330, *et seq.*

each division of which corresponds to 43″.22 of arc. The visual angle under which each division is seen is so considerable, that the divisions can readily be subdivided into tenths by estimation. The time of vibration of the magnet is 17″.78. The apparatus containing the magnet, as well as the reading telescope, are supported on stone pillars resting on solid masonry and insulated from the floor.

The observations having been undertaken by Prof. Lloyd without assistance, it was found impracticable to observe at intervals shorter than the time of vibration of the magnet bar, of which each successive elongation was accordingly noted. The time shown by the chronometer was usually noted every tenth or twelfth vibration; and thus the time of the intervening observations could be interpolated with much exactness. The error of the chronometer was obtained on the nights of the 11th, 14th, and 19th of November, by transit observations with the four-foot transit of the Observatory.

Of these observations, those made on Wednesday, Nov. 13, (8—9,) P. M. Greenwich mean time, were the most favorable for the purpose contemplated. The changes, though small, (from 5″ to 50″,) were marked and rapid, the intervals of the successive maxima and minima averaging thirty six seconds. The epoch of their occurrence seems to be determinable to between four and five seconds.

When the two sets of observations were reduced and laid down in curves, it was found that they presented no similarity; in other words, that there was *no correspondence whatever between the smaller changes of the declination at Dublin and at Philadelphia.* The determination of differences of longitude, by means of the magnet, is therefore, impracticable at such distances; but the attempt has revealed the important fact, that the irregular changes of declination, which have exhibited so marked a correspondence at the most distant stations at which simultaneous observations have been heretofore made, do not correspond on the American and European continents. Prof. Lloyd observed that much light would, ere long, be thrown upon this curious subject, by a comparison of the observations made at the Magnetical Observatory of Toronto, in Upper Canada, with those of Europe.

22. *Fossil Foraminifera in the Green Sand of New Jersey.*—Prof. J. W. Bailey, in a recent visit to the cretaceous formations of New Jersey, has brought to light the interesting fact that a large portion of the calcareous rock defined by Prof. H. D. Rogers as the third formation of the upper secondary, is made up, at the localities where he examined it, of great quantities of microscopic shells, belonging to the Foraminifera of D'Orbigny, which order includes those multilocular shells which compose a large part of the calcareous sands, &c. of Grignon and other localities in the tertiary deposits of Europe. Since the minute multilocular shells

above alluded to were discovered, Dr. Torrey and Prof. Bailey have together examined specimens of limestone from Claiborne, Alabama, and have found in them Foraminifera, of forms apparently identical with those occurring in New Jersey. None of this order except the genus Nummulite have heretofore been noticed in our green-sand formation. In this connection we may also announce the interesting discovery recently made by Prof. Wm. B. Rogers, of

A vast Stratum of Fossil Infusoria in the Tertiary Strata of Virginia.—It occurs about twenty feet in thickness, beneath Richmond, and is found to be filled with new and highly interesting forms of *marine* siliceous infusoria.* It would be interesting to have the specimens of the green-sand formation of the far west, collected by Mr. Nicollet, examined, to see if infusoria or Foraminifera may not be found in them.

An interesting paper on these subjects, by Prof. Bailey, with two plates, will appear in our next number.

23. *Discovery, in Virginia, of the regular Mineral Salt Formation.*—As salt springs and fountains are very numerous in the western and southwestern parts of the United States, it was natural to expect that mineral salt would, sooner or later, be discovered. Indeed, strata of salt, in regular position, and roofed and floored by beds of sandstone, were, some few years since, reported by the Rev. Mr. Parker,† a missionary among the Indians of the far west, as existing in abundance, in a mountain on the Salmon river, among the Rocky mountains; but no mineral salt, in the solid form, had ever been discovered in the United States proper. Now, however, we have the pleasure of announcing this discovery on the authority of the Rev. Stephen Taylor, of Abingdon, Va. His letter to us is dated March 4, 1841, and was soon followed by a large box of salt of the most indubitable character.

It was taken from a well which is "still in the process of being dug, at the salt works about eighteen miles from Abingdon, in the county of Washington," now perhaps within the bounds of Smyth county. From Prof. Wm. B. Rogers, as the geologist of Virginia, we must expect a full scientific account of this salt formation, which, however, appears to have been discovered, as we are informed, since he visited that part of Virginia. The following particulars are derived from a correspondent of Mr. Taylor, and they may not be uninteresting in anticipation of the geological report of Prof. Rogers.

Different persons vary in their report of the depth of the different wells. That of the salt-rock well does not vary much from two hundred and sixty

* See Prof. Rogers's Report on geology of Virginia, 1841, pages 38 to 42.

† In conversation with the senior editor, Mr. Parker mentions (see his travels, p. 108) seeing the salt a mile off on the left; he was too ill to climb up to it, but the Indians procured some for him, which was pure and crystallized.

feet. At about thirty feet, gypsum has been struck in all the wells, and in the salt-rock well has continued down to the solid bed of salt-rock, occasionally changing to slate and thin veins of blue soft clay. At about two hundred and thirty feet, they arrived at the first symptoms of salt, which from plaster, slate-rock and salt, gradually changed, the salt-rock assuming the predominant part.

The writer remarks, "we have for thirty feet, continued through this substance, and now find the slate-rock gradually intermixing with it." Several wells have been bored at different times, within short distances of each other. It is said, that from one of them, some years ago, salt borings were extracted; salt water was found at two hundred and twenty six feet deep.

From a single well, sufficient salt water is now extracted in twenty four hours, to afford one thousand bushels of merchantable salt, which more than supplies the demand in that region of Virginia.

The salt forwarded to us by Mr. Taylor, is highly crystalline in its structure, and except a red color, (obviously derived from iron,*) and occasional fragments of rocks mixed with it, appears to be very pure. Its taste is decidedly and purely saline without bitterness; when pulverized, the red color almost disappears and it is tolerably white. Some small pieces were perfectly white. Specimens of gypsum were enclosed in our box; they are of a very decided character—are finely granular in structure, and of a grayish white. We have no account of the other rocks found with the salt, but from the fragments intermixed presume, that sandstone and marly clays are among them.

P. S. A second letter from Rev. S. Taylor, dated May 31, states, on the authority of Mr. W. Findley, who is the proprietor of the well in which the salt is found, that in sinking the well, they penetrated earth and rock about fifty or sixty feet, when they came upon the plaster, through which they passed about one hundred and fifty or one hundred and seventy feet. They then struck upon the bed of salt, and penetrated it about fifty or sixty feet without reaching the salt water; they then abandoned the digging, applied the auger, and bored about ten feet more, as he supposes, through the salt, but the mixture of salt water renders it uncertain. The roof then appears to be gypsum, but the floor is unknown.

24. *Proceedings of the Academy of Natural Sciences of Philadelphia.* Nos. 1 and 2, for March, April, and May, 1841.—This long established and useful institution has commenced the publication of a bulletin in monthly parts, containing notices of all the important doings of the Society, with a list of donations to the library, museum, &c. This plan was adopted by the American Philosophical Society, about two years since, in

* It is now ascertained that rock-salt is sometimes colored by animalcules.

accordance with the practice of all the important European societies, and our readers have had an opportunity of judging of its effects by the full abstracts from them which have been published in this Journal.* The usefulness and energy of scientific societies depends very much on the frequency of their contact with the public, and the announcement of their proceedings at short intervals, assures the distant members and the public, of their activity in the cause of science. We shall have occasion undoubtedly to refer to these proceedings again.

25. *A Treatise on the Theory and Practice of Landscape Gardening, adapted to North America; with a view to the improvement of country residences; comprising historical notices and general principles of the art, directions for laying out grounds and arranging plantations, the description and cultivation of hardy trees, decorative accompaniments to the house and grounds, the formation of pieces of artificial water, flower gardens, etc.; with remarks on Rural Architecture;* by A. J. Downing. New York and London, Wiley and Putnam. 1841. pp. 451, 8vo.—The nature and scope of this work may be learned from the copious title-page, which, indeed, would seem almost to supersede the necessity of a table of contents. We notice, with much gratification, the unusually beautiful manner in which this interesting volume is "got up," as the phrase is. It is printed on paper of excellent quality, (for this country,) and illustrated with seventy eight wood engravings, besides the vignettes at the commencement of each chapter, and an engraving on copper in the frontispiece. Some of the wood cuts, as the figure of the Kentucky Coffee Tree, the Charter Oak at Hartford, the residence of Washington Irving, and of the author's own residence at Newburgh, New York, are very creditable specimens of the art. We regret that our limits forbid our entering, at present, even upon a cursory examination of this highly interesting and useful work, which, while it presents an encouraging view of what has already been accomplished in this country, will doubtless greatly contribute to the diffusion of correct taste, and the promotion of this elegant art amongst us. The "hundreds of individuals who wish to ornament their grounds and embellish their places, but who are at a loss how to proceed, for the want of some *leading principles*," may here meet with the information they need; and Mr. Downing's volume, the first American treatise on landscape gardening, will at once take the rank of *the* standard work upon the subject.

* These abstracts are complete in this Journal to the beginning of this year, and the Society have published them as late as June.

ACKNOWLEDGMENTS TO CORRESPONDENTS, FRIENDS AND STRANGERS.

Remarks.—This method of acknowledgment has been adopted, because it is not always practicable to write letters, where they might be reasonably expected; and still more difficult is it to prepare and insert in this Journal, notices of all the books, pamphlets, &c., which are kindly presented, even in cases, where such notices, critical or commendatory, would be appropriate; for it is often equally impossible to command the time requisite to frame them, or even to read the works; still, judicious remarks, from other hands, would usually find both acceptance and insertion.

SCIENCE.—FOREIGN.

Arsberättelse om Framstegen i Fysik och Kemi, afgifven den 31 Mars, 1838, af Jac. Berzelius, K. V. Acad. Sec. Stockholm, 1838. 8vo.

Arsberättelse om Botaniska Arbeten och Upptäckter för är 1837, afgiven den 31 Mars, 1838, af Joh. Em. Wikstrom. Stockholm, 1839. 8vo.

Arsberättelse om Technologiens framsteg, afgiven den 31 Mars, 1838, af G. E. Pasch. Stockholm, 1839. 8vo.

Kongl. Vetenskaps-Academiens Handlingar för ar 1837 and 1838. Stockholm, 1839. 8vo. These four last from M. Berzelius.

Tal om Juridisk Statistik och gneriderne för Lagstiftrimgen, Hallet uti Kongl. Vetenskaps-Academiens, den 8 April, 1840, af Grefve M. Rosenblad. Stockholm, 1840.

Tal om R. Seraphimer Ordeno Lazarettet i Stockholm, Hallet, 1 Kongl. Vetenskaps-Academiens vid Præsidii Nedlaggande den 7 April, 1838, af Dr. C. J. Ekstromer. Stockholm, 1849.

Three Lectures on Agriculture, delivered at Oxford in 1840 and 1841; by Charles Daubeny, M. D., F. R. S., &c. Oxford, 1841. 8vo. From the Author.

Proceedings of the Royal Irish Academy, No. 26. From Prof. Bache.

On the Geological Structure of the northern and central regions of Russia in Europe; by Roderick I. Murchison, F. R. S. and E. De Verneuil. London. From the Author. Three copies.

Notice of two Geological Models and Sections forming a part of the Mineral Basin of South Wales; by R. C. Taylor, Esq.; from the Geological Society's Transactions, 1830. From the Author. May, 1841.

Transactions and Proceedings of the London Electrical Society, from 1837 to 1840. London, 1841. 4to. pp. 206. From the Councillors.

Etudes sur les Glaciers, par L. Agassiz ; ouvrage accompagné d'une atlas de 32 planches, folio. Neuchatel, aux frais de l'Auteur. 1840. 8vo. pp. 346.

Description des Echinodermes Fossiles de la Suisse ; par L. Agassiz. Premiere et Seconde parties, contenant Spatangoides, Clypeastroides et Cidavides. Planches 22, 4to. pp. 101 and 108. Neuchatel, 1839 and 1840.

Mémoire sur les Moules de Mollusques Vivans et Fossiles. par L. Agassiz. Premiere partie—Moules d'Acéphales vivans. Neuchatel, 1839. 4to. pp. 48, plates 9. From the Author.

Etudes critiques sur les Mollusques fossiles, par L. Agassiz. Premiere livraison, contenant les Trigonies du Jura et de la Craie Suisses. Neuchatel, aux frais de l'Auteur. 1840. 4to. pp. 58, planches 11.

Catalogus Systematicus Ectyporum, Echinodermatum fossilium, Musei, Neocomensis, &c., auctore L. Agassiz, Necomi, Helvetorum sumptibus auctoris. 1840. 4to. ab auctore.

SCIENCE.—DOMESTIC.

Objections to the nomenclature of Berzelius, with suggestions respecting a substitute ; by Robert Hare, M. D. From the Author.

Discourse on the object and importance of the National Institution for the promotion of Science ; by J. R. Poinsett, Secretary of War. From the Author.

Exposition of the Science of Mechanical Electricity ; by Robert Hare, M. D. 8vo. 1841. From the Author.

Compendium of the Course of Chemical Instruction in the Medical Department of the University of Pennsylvania, by Robert Hare, M. D. 8vo. 1841. From the Author.

Monograph of the genus Sciurus, with descriptions of new species and their varieties. By J. Bachman, D. D., from the Magazine of Natural History for 1839. From the Author.

Proceedings of the American Philosophical Society, Nov. Dec., 1840, No. 14. From the Society.

Supplement to No. 1 of "A Monograph of the Limniades of fresh-water univalve shells of North America ;" by S. S. Haldeman. Three copies. From the Author.

Catalogue of Philosophical and Astronomical Instruments manufactured and sold by N. B. Chamberlain, Boston.

Annual Report of the State Geologist of Michigan, (Dr. Douglass Houghton,) for 1839-40. From Dr. Houghton.

Discourse by Dr. Darlington on the character, properties, and importance to man of the natural family of plants called Gramineæ. 1841. 2 copies from the Author.

Synopsis of the Course of Lectures and Instructions in Miami University, &c.; by Prof. J. W. Scott. 24mo. From the Author.

Report of the Alleghany Company's Coal Lands, Maryland; by Prof. Walter R. Johnson. From the Author.

Final Report of the Geology of New Jersey; by Prof. Henry D. Rogers. Philadelphia, 1840. 8vo. pp. 301. Two copies. From the Author.

Letters descriptive of the Bedford Springs. Philadelphia, 1836. 18mo. pp. 148. By and from Philip H. Nicklin.

Prof. Amos Eaton's Geological note-book, for the Troy class of 1841. From the Author.

Fifth Annual Report of the Geological Survey of Pennsylvania, by and from Prof. Henry D. Rogers, State Geologist. Harrisburg, Pa. 1841. 8vo. pp. 179.

Report on the Geological Survey of the State of Virginia, for 1840; by Prof. William B. Rogers, of the University of Virginia. 1841, 8vo. pp. 131. From the Author.

Annual Report of the Geologist of Maryland, J. T. Ducatel, for 1840. 8vo. pp. 59. From the Author.

Monograph of the Limniades of North America; by S. S. Haldeman. No. 2, 1841. From the Author.

Proceedings of the Academy of Natural Sciences, Philadelphia. March and April, No. 1. May, 1841, No. 2. Two copies. From the Society.

H. Milne Edwards' Anatomy and Physiology, translated from the French by Dr. J. F. W. Lane. Boston, 1841. 8vo. pp. 312. C. C. Little and J. Brown. From the Publishers.

Fossils from the Medial Tertiary of the United States; by T. A. Conrad. No. 2. May 7, 1840. From the Author.

Monography of the family Unionidæ; by T. A. Conrad. No. 12. Two copies. Philadelphia, 1840. From the Author.

Report on the manufacture of iron, addressed to the Governor of Maryland. By Prof. J. H. Alexander, Annapolis, Md. 8vo. pp. 270, with plates. From the Author.

Annual Report of the State Geologists of New York, for 1840. Albany, 1841, 8vo. pp. 184. From Dr. L. C. Beck.

Observations on magnetic intensity at twenty one stations in Europe; by Prof. A. D. Bache, LL. D. From the Author.

On the perchlorate of the oxide of ethule or perchloric ether; by Clark Hare, and Martin H. Boyè. Philadelphia, 1841. Two copies. From the Authors.

Report on the Invertebrate Animals of Massachusetts, comprising the Mollusca, Crustacea, Annelida, and Radiata, by Augustus A. Gould, M. D. in accordance with an order from the Legislature by the Commissioners on the Zoological and Botanical Survey of the State. 8vo. pp. 373, with 15 plates. Boston, 1841. From Dr. Gould.

Description of new Fossils of Tennessee, &c. by Prof. G. Troost, with lithograph figures; from the Transactions of the Geological Society of France. From the Author.

Letter of Albert Stein to S. J. Peters on the improvement of the navigation of the Mississippi. From Dr. Troost.

Dr. Troost's fifth Geological Report on Tennessee, with a colored map and section. From the Author.

MISCELLANEOUS—FOREIGN.

German Catalogues. From Messrs. Nestler & Melle, Hamburgh.

Extrait du Catalogue des livres des fonds de la Libraire de Jent et Gassmann a Soleure en Suisse.

Catalogue of a collection of rare Books (4500 vols.) to be sold in Hamburg Sept. 27, 1841, by the auctioneer Brödermann. 12mo. pp. 372. Hamburg. From Nestler & Melle.

MISCELLANEOUS.—DOMESTIC.

Inaugural Address of Thomas Corwin, governor of Ohio. From Roswell Marsh. Also, Annual Message of Governor Corwin, 1841.

Address by Charles Miner at the democratic whig festival, Wilkesbarre, Penn.

Catalogue of Western Reserve College for 1841. From Prof. St. John. Do. from Prof. Day.

Speech of Mr. Huntington of Connecticut in the House of Representatives of the United States, 1841, on the prospective preemption bill. From the Author.

Prospectus of the American Eclectic, conducted by Absalom Peters, D. D. From Dr. Peters.

Annual Report of the Superintendent and Inspector of Salt in Onondaga county, N. Y. From J. P. B. Storer.

Annual Report for 1841 of the Trustees of the Perkins Institution and Massachusetts Asylum for the Blind. From S. J. Howe, M. D.

Dr. Ticknor's Address to the candidates for degrees and licenses in the Medical Institution of Yale College, 1841.

Rev. A. R. Wright's Address before the Alumni of Western Reserve College. From Prof. St. John.

Speech of C. M. Clay, of Fayette, in the House of Representatives of Kentucky, upon the bill to repeal the law of 1832–3, prohibiting the importation of slaves into that state.

Correspondence and remarks relative to the recent attempt to exclude Unitarians from the Young Men's Bible Society of Cincinnati. From T. C. Lea.

Semi-Annual Catalogue of Westfield Academy, 1840–41. From A. Parish.

Twenty Fourth Annual Report of the American Colonization Society.

Twentieth Annual Report of the Board of Directors of the Mercantile Library Association, New York, 1841. 2 copies.

Otis, Broaders & Co. Literary Advertiser, Boston.

Second Annual Report of the Directors and Superintendent of the Ohio Lunatic Asylum, 1841. From E. M. Sill. Do. from Roswell Marsh.

Catalogue of British, French, and American Books, for sale by Wiley & Putnam, New York.

Supplement to the General Catalogue of the Library of the Linonian Society, Yale College, New Haven.

Catalogue of Books for sale by Little & Brown, Boston.

American Tract Magazine, March, 1841.

Yale Literary Magazine, Nos. 3 and 4. From the editors.

A Report of the special agent of the lead mines relative to the sale or future management of the mineral lands of the United States. From W. W. Boardman, M. C.

Dr. Allen on Mental Philosophy. From the author.

Southern Literary Messenger, with a commendatory notice of the American Journal of Science. By and from Lt. Maury, Richmond, Virginia.

Catalogue of the Philorhetorian Society.

Description of the Missourium. From Geo. Silliman.

Catalogue of the officers and students of the Louisville Medical Institute. From H. C. Banks.

Bulletin of the Proceedings of the National Institution for the promotion of Science. From Francis Markoe, Jr.

Eighth Annual Report of the Trustees of the State Lunatic Hospital at Worcester, 1841. From Dr. Woodward.

Address on the study of Natural History, by J. G. Morris, and a Poem on the Sovereignty of Mind, by John N. McJilton. Baltimore, 1841. From the Authors.

Address by Daniel Ullman, delivered at the Broadway Tabernacle, N. Y. From the Author.

Register of the Officers and Cadets of the Virginia Military Institute at Lexington, Va. 1840.

Mr. Cleveland's dedication sermon, Stoneham. From the Author.

The proper test of Christian character, by Henry Colman. Boston, 1824. From the Author.

Lecture on Phrenology, by Frank Hamilton, M. D. Rochester, 1841. From the Author.

Report of the engineer upon the several definite locations of the Hartford and Springfield Railroad. From Charles Buck.

Eulogy on Washington, by Rev. D. C. Banks, Louisville, Kentucky, 1841. From the Author.

Sermon on the death of Lt. J. A. Underwood and midshipman Henry, of the U. S. N. by Jared J. Elliot, chaplain. Delivered on board the U. S. ship Vincennes. Printed at the mission press, Honolulu, Oahu. From J. D. Dana, of the Exploring Expedition.

Sermon on the death of Joseph May, aged 81, by F. W. P. Greenwood. From S. May.

Catalogue of the University of Pennsylvania, for Yale College Library. From P. H. Nicklin.

Report made to the Trustees of the University of Pennsylvania, by P. H. Nicklin, for Yale College Library. From P. H. Nicklin.

Report of the committee of investigation appointed by the Stockholders of the Bank of the United States.

Remarks by President Quincy, of Harvard University, on the voluntary system in the studies of Greek and Latin. From President Quincy.

Syllabus of a course of popular lectures on Physiology, by Reynell Coates, M. D. From Dr. Coates.

Discourse before the Georgia Historical Society, Savannah, by Wm. Bacon Stevens, M. D. From the Author.

Funeral Sermon delivered in the Presbyterian church, Washington, on the Sabbath after the decease of William H. Harrison, late President of the United States, by Rev. C. Van Rensselaer, of Burlington, N. Y. From the Author.

Twenty Second Annual Report of the Directors of the New York Institution for the Instruction of the Deaf and Dumb, 1840. From J. H. Pettingill.

Commendations of Dr. N. Webster's Books.

Catalogue of English and American Books to be sold at auction May 3, by Bangs, Richards & Platt.

Prince's select Catalogue of double Dahlias, 1841.

Perkins' Higher Arithmetic. From the Author.

Eulogy on William Henry Harrison, by Rev. J. Richards. From the Author, Windsor.

Address before the Boston Natural History Society, by J. E. Teschemacher, May 5, 1841. 2 copies from the Author.

The History of Harvard University, by Josiah Quincy, LL. D. 2 vols. 8vo. C. C. Little and James Brown. From the Author.

History of the Belville Settlement, by S. P. Hildreth, M. D. Cincinnati, 1839. From the Author.

An introductory lecture before an Education Convention, by John T. Plummer, M. D. Springfield, Ind. May 11, 1841. From the Author.

New Hampshire Medical Institution Annual Circular, Hanover, N. H. 1841. From Prof. Hubbard.

Mr. Vinton's Sermon on the National Fast Day, May 14, 1841. From Lt. Wm. Rogers Taylor, U. S. N.

Seventeenth Annual Report of the Directors of the Hartford Insane Retreat, Hartford, 1841. From Dr. A. Brigham.

The History of Harvard University, by President Josiah Quincy, LL. D. 2 vols. 8vo. with illustrations. 2 copies, one for the Library of Yale College. From the Author.

History of Long Island, by Benjamin F. Thompson, Esq. 1 vol. 8vo. with illustrations. From the Author.

Sketches of Iowa, with a map, by John B. Newhall, Esq. From the Author.

SPECIMENS.—FOREIGN.

Seventy Mineralogical Specimens from Faroe and the north of Europe. From Prof. George Forchhammer, of Copenhagen.

Two hundred Geological and Mineralogical Specimens. From Dr. Fr. Tamnau, Berlin, Prussia.

Minerals of Nova Scotia. From Rev. Costar Parrsborn, N. S.

Models and specimens of several Saurian reptiles from the lias of England. From Wm. Stutchbury, Esq. Bristol Institution.

Agates and Carnelians from Singapore. From Rev. J. T. Dickinson.

Fossil Fish of Mt. Libanus, and Salt from near the Dead Sea. From Rev. Eli Smith.

Minerals from the Sandwich Islands. From Rev. Mr. Judson.

Minerals from the vicinity of Mt. Vesuvius. From J. E. Teschemacher, Esq.

SPECIMENS.—DOMESTIC.

Native Steel Ore of Duane, N. Y. From M. H. Read.

Tubular Concretions of Iron sand, from Florida coast. From Lieut. James Gerry.

Phosphate of Uranium, from Chesterfield, Mass. From Mr. J. E. Teschemacher.

Red Pipe Stone, (Catlinite,) from Côteau des Prairies—also quartz geodes. From J. N. Nicollet, Esq.

A box of fine Rock Salt, from Abingdon, Va. From the Rev. Stephen Taylor. (See p. 214, of this number.)

NEWSPAPERS.—DOMESTIC.

The Country Advertiser.—Marietta Intelligencer. From the Editor.—Union Agriculturalist. From the Editors.—New England Christian Advocate.—St. Louis Republican, with notice of La Motte lead mines.—New York Journal of Commerce.—The Future.—Rochester Daily Advertiser. From Thomas H. Hyatt, Editor.—Nantucket Enquirer. From S. M. Jenks, Editor.—American Standard, Albion, N. Y. From T. C. Strong, Editor.—Ohio Observer. From E. P. Barrows, Editor.—Le Roy Gazette. From C. B. Thomson, Editor.—Fredonia Censor. From E. Winchester, Editor.—Charleston Observer, Charleston, South Carolina.—Cincinnati Gazette.—Constitution, Middletown. From Professor Johnston, with an account of the explosion of the steam-boiler in the sash and blind factory of Messrs. Pike & Perry.—Boston Daily Advertiser.

From Dr. Wyman.—Literary Pearl, Charlton, Saratoga Co. N. Y. From the Editor, with an account of a meteor.—Republican Standard, Bridgeport. From James H. Linsley.—Savannah Daily Republican. From Dr. Watkins.—Macon Georgia Telegraph.—Vicksburg Daily Whig. From George W. Niles, with a meteorological Table.—Rochester Republican. From the Editor.—Greensburg Sentinel.—Scioto Gazette.—Indiana Gazette. From Dr. Plummer.—Louisville Journal. From H. C. Banks.—St. Louis New Era, February 27, 1841, containing meteorological table for December, 1840. From C. C. Whittelsey.—The Constitutionalist, Bath, Maine, March 10, 1841, notice of parhelia.—York County Herald.—"Olive Leaf," New York, Providence, Boston.—New York Evangelist.—Northern Light, Albany.—Mohawk Advertiser. From J. A. Lapham.—Charleston Courier.—Rutland Herald.—Bloomington Herald, I. T. From J. S. Parvin.—Maumee River Times.—Cincinnati Chronicle.—Vermont Chronicle.—Greensburgh Sentinel, Pa.—Milwaukie Sentinel.—Cattskill Messenger.—The Constitution, Middletown. From Dr. Barratt with a statement showing the falls of snow at Middletown, Conn. from Oct. 1840, to April, 1841.—Democratic Herald, Ann Arbor.—The Daily Picayune, New Orleans.—Sentinel and Witness, Middletown, with an obituary notice of Dr. Miner, by Dr. Barratt.—Christian Observer, Philadelphia.—Jewett's Advertiser.—Kentucky Farmer, containing an analysis of Kentucky soil, by Dr. Peter.—Maumee River Times, with a notice of the Am. Journ.—New York Sun.—Louisville Public Advertiser.—Daily Evening Gazette, St. Louis.—Greensburg Sentinel.—The Boston Atlas.—Self Instructor, Extra, New York.—The Free Press, Xenia, Ohio.—Niles' National Register, Baltimore.—Union Agriculturalist, Chicago.—Leonard Town Herald.—Commercial Advertiser, Buffalo. From R. W. Haskins, with a notice of that gentleman's astronomy for schools.—United States, Philadelphia.—New York American, June 16, 1841.—Zion's Advocate, June 16 and 23, 1841, with a notice of the American Journal of Science.—Various numbers of the Ohio Observer, containing Meteorological Tables. From Prof. E. Loomis.

NEWSPAPERS—FOREIGN.

Halifax (Nova Scotia) Morning Post, with an account of the Mechanics' Institute.—Oxford University, England, with report of the communication of Prof. Buckland on the animalculæ in limestone.—Sussex Advertiser.—Montreal Transcript, June 3, 1841.—Anti-Slavery Reporter, London, in series.—The Witness, Edinburgh, extra royal sheets for May 29, and June 1, 1841. From John Dunlop, Esq.

ADVERTISING SHEET

OF THE

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Boston, October, 1841.

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} *Local Committee.*

Prof. B. SILLIMAN, LL. D., &c. to deliver the opening address.

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October, 1841.

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ERRATA.

In Mr. Hodge's article on the tertiary of the Southern States, are the following mistakes.—For Cytherea reporta, passim, read Cytherea reposta.

" Plagiostoma palagicum, read P. pelagicum, pa. 337 l. 10 and 11.

" that the home-made, read and the home-made, pa. 341 l. 11 fr. bot.

" Venus mercinaria, read V. mercenaria, pa. 344 l. 7 fr. bot.

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